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Consumer Behavior And Firm Marketing Strategy Under Assortment Expansion

Abstract

This dissertation studies two types of assortment expansion strategies: category expansion and the launch of a new service. We first explore the impact of category expansion on customer demand and firm pricing strategy. We theoretically and empirically demonstrate the "dark side" of category expansion: the price sensitivity of existing categories may increase. We develop a model of multi-category purchase with travel costs to capture customers' preference for one-stop shopping—the primary motivation for category expansion. We then apply the model to study the price sensitivity of grocery categories after liquor was introduced to private stores in the state of Washington due to a deregulation policy. Contrary to conventional wisdom, we find price sensitivity increases in categories low in demand and complementary to liquor. These changes, if ignored, would lead to a significant profit loss. Next, we study the pricing strategy of the newly introduced category post assortment expansion. Specifically, we examine how and why liquor prices change after privatization in the state of Washington. We propose five mechanisms and develop a framework to analyze their price effect using counterfactual simulations. Our results suggest that, contrary to the policymaker's expectation, competition does not lower liquor prices. Rather, liquor prices surge because of the high license fees. Finally, we examine the impact of a subscription program on customer purchases. We adopt a quasi-experimental method to identify individual-level treatment effects. We find the subscription program leads to a large increase in customer purchases. The effect of the subscription program is economically significant, persistent over time and heterogeneous across customers. Interestingly, the program's economic benefits only explain a third of the effect size. Evidence suggests that customers commit a sunk cost fallacy in that they increase purchases to justify their subscription decisions.

Degree Type

Dissertation

Degree Name

Doctor of Philosophy (PhD)

Graduate Group

Marketing

First Advisor

Eric T. Bradlow

Second Advisor

Ron Berman

Subject Categories

Advertising and Promotion Management | Marketing

CONSUMER BEHAVIOR AND FIRM MARKETING STRATEGY UNDER
ASSORTMENT EXPANSION

Qi Yu

A DISSERTATION

in

Marketing

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

Degree of Doctor of Philosophy

2021

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CONSUMER BEHAVIOR AND FIRM MARKETING STRATEGY UNDER
ASSORTMENT EXPANSION

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ACKNOWLEDGMENT

First and foremost, I would like to thank my advisors Professors Eric Bradlow and Ron Berman for their inspiration, guidance, and support. They always encourage me to ask important questions yet never lose focus on any detail. They guide me to make progress continuously to achieve the goals that seemed beyond reach initially. It is my great privilege to have them as my advisors.

I am deeply indebted to my committee member, Professor Raghu Iyengar, who has been a mentor throughout these five years. He has provided numerous valuable professional and personal advice and has helped my development as a researcher. I am also grateful to other faculty members at Wharton and my collaborators for their feedback and support during my Ph.D. journey.

My sincere thanks go to my fellow Ph.D. students and friends, Vladimir, Yuhao, Esther, Yupeng, Feng, Rachel, and Minyue, who have provided stimulating discussions, constructive comments as well as happy distractions to rest my mind outside of my research.

Finally, my greatest appreciation goes to my family. None of my achievements would have been possible without them.

ABSTRACT

CONSUMER BEHAVIOR AND FIRM MARKETING STRATEGY UNDER ASSORTMENT EXPANSION

Qi Yu

Ron Berman

Eric T. Bradlow

This dissertation studies two types of assortment expansion strategies: category expansion and the launch of a new service. We first explore the impact of category expansion on customer demand and firm pricing strategy. We theoretically and empirically demonstrate the “dark side” of category expansion: the price sensitivity of existing categories may increase. We develop a model of multi-category purchase with travel costs to capture customers’ preference for one-stop shopping—the primary motivation for category expansion. We then apply the model to study the price sensitivity of grocery categories after liquor was introduced to private stores in the state of Washington due to a deregulation policy. Contrary to conventional wisdom, we find price sensitivity increases in categories low in demand and complementary to liquor. These changes, if ignored, would lead to a significant profit loss. Next, we study the pricing strategy of the newly introduced category post assortment expansion. Specifically, we examine how and why liquor prices change after privatization in the state of Washington. We propose five mechanisms and develop a framework to analyze their price effect using counterfactual simulations. Our results suggest that, contrary to the policymaker’s expectation, competition does not lower liquor prices. Rather, liquor prices surge because of the high license fees. Finally, we examine the impact of a subscription program on customer purchases. We adopt a quasi-experimental method to identify individual-level treatment effects. We find the subscription program leads to a large increase in customer purchases. The effect of the subscription program is economically significant, persistent over time and heterogeneous across customers. Interestingly,

the program's economic benefits only explain a third of the effect size. Evidence suggests that customers commit a sunk cost fallacy.

TABLE OF CONTENTS

ACKNOWLEDGMENT	iii
ABSTRACT	iv
LIST OF TABLES	ix
LIST OF ILLUSTRATIONS	x
CHAPTER 1 : Introduction	1
CHAPTER 2 : The Dark Side of Category Expansion: Will (and Which) Existing Ones “Pay the Price”?	4
2.1 Introduction	4
2.2 A Stylized Model for Category Expansion	9
2.3 Empirical Context and Data	14
2.4 Demand Model	24
2.5 Results	30
2.6 Conclusion and Discussion	41
CHAPTER 3 : Does Privatization Lead to Lower Liquor Prices: Evidence from the State of Washington	44
3.1 Introduction	44
3.2 Literature Review	47
3.3 Institutional Background and Data	49
3.4 Empirical Framework	54
3.5 Results	59
3.6 Conclusion and Discussion	62

CHAPTER 4 : The Impact of Subscription Programs on Customer Purchases . . .	64
4.1 Introduction	64
4.2 Subscription Business	68
4.3 Empirical Context and Data	70
4.4 Method	72
4.5 Robustness Checks	93
4.6 Profitability of the Program	95
4.7 Conclusions	99
CHAPTER 5 : Conclusion	101
APPENDIX	103
BIBLIOGRAPHY	123

LIST OF TABLES

TABLE 1 :	Changes in Price Sensitivity in Different Parameter Regions	14
TABLE 2 :	Summary Statistics	17
TABLE 3 :	Cross Tabulation of Grocery and Liquor Purchases	18
TABLE 4 :	Changes in Purchase Behavior after Assortment Expansion	20
TABLE 5 :	Regression Results	22
TABLE 6 :	Changes in Marketing Mix after Assortment Expansion	24
TABLE 7 :	Model Fit	31
TABLE 8 :	Model Estimates	33
TABLE 9 :	Bundle Utility and Cross-Price Sensitivity	35
TABLE 10 :	Changes in Price Sensitivity and Its Heterogeneity	38
TABLE 11 :	Percentage Profit Loss When the Changes in Price Sensitivity are Ignored	42
TABLE 12 :	Changes in Net-of-fees Price, Demand, and Price Index	53
TABLE 13 :	Own- and Cross-Price Elasticity	56
TABLE 14 :	Counterfactual Simulation Setup	57
TABLE 15 :	Summary Statistics of Members versus Non-members	75
TABLE 16 :	Covariates for Generalized Random Forests.	81
TABLE 17 :	Treatment Effects	84
TABLE 18 :	Non-economic Effects	87
TABLE 19 :	Regression Results	93
TABLE 20 :	Treatment Effects across Cohorts.	95
TABLE 21 :	Non-economic Effects across Cohorts	96
TABLE 22 :	Profitability and Characteristics by Segment	98
TABLE 23 :	Empirical Tests for Parallel Time Trends	105

TABLE 24 : Parameter Recovery	107
TABLE 25 : Placebo Tests	108
TABLE 26 : Treatment Effects with $\log(Y_{it})$	123
TABLE 27 : Non-economic Effects with $\log(Y_{it})$	124

LIST OF ILLUSTRATIONS

FIGURE 1 :	S-shaped Demand Curve	6
FIGURE 2 :	Change in Price Sensitivity after Assortment Expansion	12
FIGURE 3 :	Predicted and Observed Individual-level Co-purchase	32
FIGURE 4 :	Distribution of Transportation Costs	34
FIGURE 5 :	Hypothesized Changes in Price Sensitivity across Categories	36
FIGURE 6 :	Changes in Price Sensitivity at the Individual Level Across Categories	39
FIGURE 7 :	Washington Liquor Prices in 2012	51
FIGURE 8 :	Decomposition of Price Changes	59
FIGURE 9 :	Distribution of the Price Effects	61
FIGURE 10 :	Customer Purchase of Members verses Non-members	74
FIGURE 11 :	Covariate Balance Before and After Weighting	82
FIGURE 12 :	Distribution of the Treatment & Non-economic Effect	89
FIGURE 13 :	Scatter Plot of Profit and Cost	97
FIGURE 14 :	Customer Purchases when $\tau = 0, \beta_1 = 0, \beta_2 = 0$	117
FIGURE 15 :	Customer Purchases when $\tau = 0, \beta_1 = -3, \beta_2 = 0$	118
FIGURE 16 :	Customer Purchases when $\tau = 0, \beta_1 = 0, \beta_2 = 3$	118
FIGURE 17 :	Customer Purchases when $\tau = 18, \beta_1 = 0, \beta_2 = 0$	119
FIGURE 18 :	Parameter Recovery of τ	121
FIGURE 19 :	Parameter Recovery of β_1	121
FIGURE 20 :	Parameter Recovery of β_2	122

CHAPTER 1 : Introduction

Retailers exploit a variety of options to grow their profits. Many invest substantial effort in product assortment expansion to generate incremental demand. Assortment expansion may take the form of a deeper single product assortment (i.e., a larger number of variations), a wider variety of products within a category (i.e., a larger number of different products), or category expansion (i.e., new categories in the store). The last approach is increasingly common among retailers. For instance, retailers that did not traditionally provide fresh groceries (e.g., Target and Walgreens) have started offering fresh food items to boost sales. In the other direction, Kroger, originally a grocery store, introduced an apparel section in 2014 in response to increased competition.

Other retailers go beyond expanding their product assortments and launch new services to retain customers. Subscription programs, which offer customers access to a variety of benefits and services for an upfront payment, are popular in recent years. For example, Amazon Prime offers members free shipping, audio and video content, as well as member-exclusive discounts for \$119 per year. Many other retailers, such as Barnes & Noble, Sephora, and Alibaba, have programs of a similar payment structure, and their benefits range from product samples to free returns. As the relevance and popularity of subscription programs grow, it is of managerial interest to examine the causal effects of such programs on customer behavior and investigate the underlying drivers for their success.

This dissertation seeks to understand consumer behavior and firm marketing strategy under these two types of expansions. The next two chapters study customer price sensitivity and retailer pricing strategy after category expansion. In Chapter 2, we challenge the conventional belief that “more is better.” We theoretically and empirically demonstrate that the price sensitivity of existing categories may increase after assortment expansion. First, we develop an analytical model and show that price sensitivity is endogenously determined by product assortments. When an existing category is low in demand and complementary

to the new one, its demand increases after assortment expansion as customers value the convenience of one-stop shopping. However, this increase in demand also suggests a movement towards the steeper part of the demand curve and price sensitivity increases. We empirically test the model predictions in a natural experiment setting. In June 2012, the state of Washington ended its monopoly in the liquor markets. Qualified private licensees, including many grocery stores, started to carry liquor. We estimate the changes in price sensitivity for existing categories in these stores using a multi-category demand model. The model builds on a bundle utility framework (e.g., Bradlow and Rao, 2000; Gentzkow, 2007) and incorporates correlated preferences, price endogeneity, as well as heterogeneity across customers. We find that in two out of the six existing categories, price sensitivity increases after assortment expansion. Further, there is substantial heterogeneity across customers. Consistent with the theoretical predictions, the increase in price sensitivity is considerable when the customer has a weak preference for a category and that category is a complement for liquor. Counterfactual simulations suggest that retailers could have a profit loss as high as 2.2% if the changes in price sensitivity were ignored.

In Chapter 3, we turn our attention to the newly introduced category and study the pricing strategy post assortment expansion. In the same context, we examine how and why liquor prices change after the privatization in the state of Washington. We propose five mechanisms that could affect liquor prices. First, the privatization of liquor introduces competition, which could lead to lower prices. Second, as will be discussed in Chapter 2, privatization offers customers the convenience of one-stop shopping and increases price sensitivity. Third, grocery stores selling liquor internalize the cross-price effects between these two types of products and set prices differently from the stand-alone liquor stores. Fourth, the new fees that come with the privatization could be passed through to consumers. Finally, grocery stores and stand-alone liquor stores may have different sizes and types of liquor selections. We develop a unified framework to analyze the price effect of each mechanism using a series of counterfactual simulations. Our results suggest that contrary to the policymaker's expectation, introducing competition does not lower prices. On the other hand, multi-

category retailers lower prices to leverage its positive impact on the demand for other categories. However, this price reduction is outweighed by the price increase due to the high license fees. On net, liquor prices increase after the privatization.

In Chapter 4, we study the impact of subscription programs on customer purchases. We ask if subscription programs have a causal impact on customer purchase and if the effect goes beyond the economic benefits offered to the customers. We leverage a quasi-experimental design to control for self-selection and identify the effect at the individual level. Our empirical model combines a difference-in-differences approach with a generalized random forests procedure that matches each member of the subscription program with comparable non-members. We find that the subscription program leads to a large increase in customer purchases. The effect of the subscription program is economically significant, persistent over time, and heterogeneous across customers. Interestingly, only one-third of the increase in purchases is due to the program's economic benefit. We posit that customers experience a sunk cost fallacy in that they increase their purchases to justify their subscription decisions. We provide evidence supporting this mechanism.

CHAPTER 2 : The Dark Side of Category Expansion: Will (and Which) Existing Ones “Pay the Price”?

2.1. Introduction

Many retailers place significant effort into assortment expansion to increase revenue. One observation is that the average number of SKUs a retailer offers is exploding, especially when the retailer moves online (Hübner and Kuhn, 2012). Assortment expansion may be in the form of a deeper single product assortment (larger number of variations), a wider variety of products within a category (larger number of different products), or category expansion (new categories in the store). This last approach is increasingly common among retailers. For instance, in the last decade, retailers that did not traditionally provide fresh groceries, including discount stores (e.g., Target) and drug stores (e.g., Walgreens), have started offering fresh food items to boost sales.¹ In the other direction, in 2014, Kroger, originally a grocery store, introduced an apparel section to its Marketplace stores nationwide in response to competition.² One belief held by many retailers is that category expansion is always beneficial, especially if the new category complements the existing ones.

Despite the conventional belief that category expansion is always desirable, past marketing research has provided little insight on whether that is true. With the exception of Goli and Chintagunta (forthcoming), existing literature focuses on product-level expansion and provides mixed evidence on whether a larger assortment is beneficial. On the one hand, advocates of a larger assortment consider the benefit of one-stop shopping and find that a larger assortment will increase store visits and sales (Kalyanam et al., 2007; Briesch et al., 2009; Thomassen et al., 2017). On the other hand, a larger and deeper assortment can be less desirable, as customers may have higher evaluation costs and expectations (Iyengar and

¹“Shopping at Target? Now You Can Pick Up a Dozen Eggs,” available at <https://www.nytimes.com/2010/12/17/business/media/17adco.html> (accessed April 5, 2021).

“Walgreens to Add Fresh Food to Hundreds of Stores,” available at <https://www.cspdailynews.com/foodservice/walgreens-add-fresh-food-hundreds-stores> (accessed April 5, 2021).

²“More Kroger Marketplaces Adding Clothing,” available at <https://www.cincinnati.com/story/money/2014/08/07/kroger-marketplace/13750905> (accessed April 5, 2021).

Lepper, 2000; Kuksov and Villas-Boas, 2010). Some research suggests that certain firms should even strategically offer *less* variety to increase product differentiation and to avoid intensive price competition (Martinez-Giralt and Neven, 1988).

In this chapter, we study the impact of category expansion on customer demand. We argue that introducing of a new product category may sometimes have a surprising effect and “dark side”—it may lead to increased price sensitivity of demand in the existing categories, i.e., an increase in price will be associated with a larger decrease in demand than before. When customers are more price-sensitive, the (optimal) retailer will only be able to charge a smaller margin. In addition, we show that if the changes in price sensitivity are not taken into account, the retailer will price sub-optimally and thus yield lower profits.

To understand why assortment expansion may lead to increased price sensitivity in an existing category, consider a customer with logit demand for that category (Figure 1). The S-shaped demand curve implies that if the utility of the category is extremely high (point *A*) or extremely low (point *D*), a small increase in price has little effect on demand. In contrast, the customer is the most price-sensitive when the utility of the category and the alternatives are balanced. The introduction of a new category offers the customer the convenience of one-stop shopping and increases the demand for the existing category if they are complements. However, this increase in demand also suggests a movement towards the “tipping point” on the demand curve and an increase in price sensitivity if the initial demand for the existing category is low (point *C*). Similarly, as illustrated in Figure 1, the existing product categories can be classified into four groups depending on their baseline preference and whether they are complements or substitutes for the new category. In particular, the price sensitivity of categories in group B (strong preference and substitute) and C (weak preference and complement) increases. We formalize the above mechanism with a stylized model, which gives sharp predictions of the conditions under which price sensitivities of existing categories increase after assortment expansion.

We empirically test these predictions in a natural experiment setting. Our analyses leverage

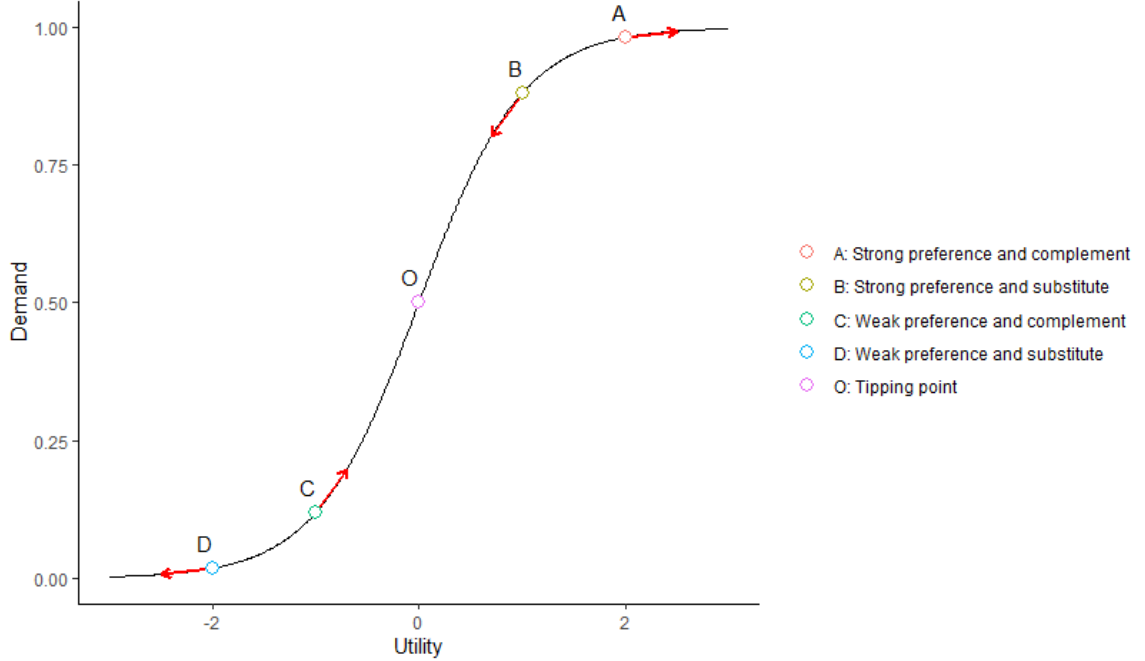


Figure 1: S-shaped Demand Curve

the state of Washington's deregulation of the liquor market. Before May 2012, the state of Washington held a monopoly on hard liquor sales. From June 1, 2012 onward, private retailers with premises larger than 10,000 square feet were allowed to enter the market, creating variation in the assortments of these stores over time.

We examine the change in price sensitivity of six existing food categories in the affected stores using both reduced-form and Bayesian Hierarchical Choice models. Through a difference-in-differences analysis, we find that the average price sensitivity of existing categories increases after a store started to carry liquor. Moreover, the change in price sensitivity depends on customer and category characteristics. Consistent with the theoretical predictions, when a customer has low demand for an existing category that complements liquor, the customer's price sensitivity of that category increases.³ Next, to systematically study the differential changes in price sensitivity across product categories and customers, we extend the analytical model and build a Bayesian Hierarchical Choice model of multi-

³In this context, there is no such category that complements liquor and has a high demand.

category purchase with transportation costs. We explicitly model customers' heterogeneous preferences for each category and co-purchase. The model also accounts for confounders that could yield a spurious change in price sensitivity that are not due to the assortment expansion, including customers' correlated preferences across categories and price endogeneity. The estimates from the model suggest that customers value the convenience of one-stop shopping. The average willingness-to-pay for one-stop shopping is \$5.6. The value of one-stop shopping is especially large for customers who reside in areas with a low density of liquor stores where transportation costs are likely higher. Based on the estimates from the model, we examine the change in price sensitivity and its economic significance. The results confirm those from the reduced-form analyses and reveal that the increase in price sensitivity is notable. In the frozen food category, customers increase their price sensitivity by 1.8% on average and more than 80% of customers experience such an increase. In the alcoholic beverage category, customers increase their price sensitivity by 2.6%, and 97% of customers become more price-sensitive post assortment expansion. Averaged across all existing categories, individual-level price sensitivity increases in 42% of the cases. Further, there is substantial heterogeneity across customers. Interestingly, the increase in price sensitivity is not necessarily the largest for customers with the highest transportation cost. For example, in the frozen food category, customers who live in the area with the highest density of liquor stores have the largest increase in price sensitivity. This is because these customers also have the weakest preference for frozen food. Overall, if these changes in price sensitivity were ignored, retailers could have a total profit loss as high as 2.2%.

This research contributes to several streams of literature. Methodologically, our work is closely related to the growing literature on multi-category demand, in particular, modeling the demand of complements (Manchanda et al., 1999; Bradlow and Rao, 2000; Gentzkow, 2007; Song and Chintagunta, 2007; Mehta and Ma, 2012; Thomassen et al., 2017). For instance, Manchanda et al. (1999) use a multivariate probit framework to model purchases across different categories and identify a correlation in preferences across categories. While using different utility specifications, Song and Chintagunta (2007), Mehta and Ma (2012),

and Thomassen et al. (2017) model multi-category purchases using direct utility approaches. However, these models become quickly intractable as the number of products increases. Gentzkow (2007) studies complementarity between print and online newspapers in a panel data setting and shows that complementarity can be separately identified from product utility correlation. Our model extends Gentzkow (2007) in that it is flexible and takes into account various confounders, enabling us to obtain unbiased estimates of the demand parameters.

From a substantive perspective, our work adds to the ongoing debate regarding whether assortment expansion is beneficial (Boatwright and Nunes, 2001; Borle et al., 2005; Kalyanam et al., 2007). Specifically, we establish the effect of assortment expansion on price sensitivity in a quasi-experiment setting. Our results suggest that assortment expansion is not always beneficial - it may increase price sensitivity for existing categories. Our work also complements the recent literature on category expansion (elimination). For instance, Goli and Chintagunta (forthcoming) study the spillover effect of the exit of tobacco category in one chain on the sales of nearby stores. They find an increase in sales from both tobacco and non-tobacco categories in nearby stores. We find an increase in price sensitivity across several existing product categories, which affects the retailer’s optimal prices.

Finally, this research also contributes to the growing literature on the privatization of the liquor market. Except for Seo (2019), the existing literature focuses on the effect of deregulation on the new category and neglects the spillover to existing categories. Illanes and Moshary (2020) exploit the discontinuity in private liquor license eligibility in store size in the State of Washington and study retailers’ liquor pricing and assortment decision given different market structures. Huang et al. (2020) examine private retailers’ learning behavior when they are inexperienced after the privatization of liquor and learn about the demand for liquor in Washington. Seo (2019) evaluates the gains in consumer welfare due to change in firm scope. In contrast to the prior literature that largely focuses on the new category, liquor, this research assesses the change in price sensitivity for existing categories.

Our findings have important implications for pricing and assortment planning. The results suggest that demand models which assume price sensitivity to be time-invariant and independent of the assortment may poorly approximate customer behavior in practice. Retailers should thus update the demand forecasts and re-optimize price levels after assortment expansion; otherwise, they might have a loss in expected profit. Taken a step further, this spillover should also be taken into account when retailers plan for their assortments.

The rest of the chapter is organized as follows. In Section 2.2, we present an analytical model to understand the effect of category expansion on price sensitivity. Section 2.3 describes the empirical setting in greater detail and explains why it is appropriate for our research. We then present descriptive evidence that price sensitivity indeed increases after assortment expansion. Section 2.4 develops a formal demand model that estimates the change in price sensitivity while accounting for various confounds. Section 2.5 presents the model estimates. Section 2.6 discusses the change in price sensitivity and implications for retailers' pricing strategy through counterfactual predictions. We conclude by reviewing the limitations of this research and discussing directions for future work.

2.2. A Stylized Model for Category Expansion

In this section, we demonstrate that assortment expansion may lead to an increase in price sensitivity in a stylized analytical model. In what follows, we start with an example where a store started with one existing category and adds a new category to its assortment. We then discuss the predictions when there is more than one pre-existing category.

2.2.1. *Two categories*

Consider a market with two product categories A and B , and a store G . Before assortment expansion, store G carries only category A , and category B is sold in a separate store. After assortment expansion, the focal store G carries both A and B . Assume that a customer has unit demand for each category. In a shopping trip, the customer chooses among all possible bundles (combinations) of categories $b \in \{0, A, B, AB\}$ and picks the one that gives her the

highest utility. The utility of each bundle is given by

$$\begin{aligned}
u_0 &= \epsilon_0 \\
u_A &= \alpha_A - \beta p_A + \epsilon_A \\
u_B &= \alpha_B - \beta p_B + \epsilon_B \\
u_{AB} &= \alpha_A + \alpha_B - \beta(p_A + p_B) + \Gamma_{AB} - s + \epsilon_{AB}
\end{aligned} \tag{2.1}$$

where α_A and α_B are mean utilities for the two product categories, p_A and p_B are prices, β is a common price coefficient, and ϵ_0 , ϵ_A , ϵ_B , and ϵ_{AB} are unobserved variations in utility that are assumed to follow i.i.d. extreme value distributions. The term Γ_{AB} captures the difference in utilities from co-purchasing two categories and purchasing them independently. In this model with two categories, the sign of Γ_{AB} has a one-to-one mapping to complementarity defined by cross-price effects (Gentzkow, 2007). Specifically, if $\Gamma_{AB} > 0$, the two categories are complements, i.e., increasing the price of one category leads to a decrease in demand for another. If $\Gamma_{AB} < 0$, the two categories are substitutes. Finally, the customer may incur a transportation cost in the shopping trip. We normalize the transportation cost of traveling to one single store to zero. If the customer purchases both categories in the same shopping trip before assortment expansion, she must visit two stores and pay a transportation cost $s = s_0$. This transportation cost can include extra travel cost or time cost for the shopping trip and any psychological cost of visiting multiple stores. After assortment expansion, we assume that the customer patronizes store A so $s = 0$, i.e., there is no extra cost of purchasing both categories within the same trip.⁴

Let $D_b(s)$ denote the probability of purchasing the bundle b given the customer pays a transportation cost of s . Let $Q_c(s)$ denote the total demand for category c ($c = A, B$). The total demand consists of the demand for the singleton bundles and the bundle with two categories, e.g., $Q_A(s) = D_A(s) + D_{AB}(s)$. Let $e_b(s) = \frac{\partial Q_c(s)}{\partial p_c}$ denote the price sensitivity of the (total) demand for category c . For the ease of illustration, let v_b denote the deterministic

⁴This assumption would also arise endogenously if store G and the outside store are identical in quality and pricing of category A . In this case, one-stop shopping dominates two-stop shopping.

part of bundle b 's utility. We now derive the demand and price sensitivity for the existing category A under different scenarios.

With the benefit of one-stop shopping, not surprisingly, the customer's demand for the existing category A increases after assortment expansion. However, the price sensitivity of category A also increases when the initial purchase probability of A is below $1/2$. The intuition is as follows. The customer's demand is in an S shape (Figure 1), which implies that if the utility of a product category is extremely high (point A) or extremely low (point D), a small increase in price has little effect on its demand. In contrast, the customer is the most price-sensitive when the utilities of different categories are balanced (point O). After assortment expansion, the customer no longer pays the transportation cost and gets higher utility from the category. If the customer has a relatively weak preference for the category, her demand will move to the steeper part of the curve and she becomes more price sensitive. Similarly, if the customer has a strong preference for the category, she will become less price-sensitive. Proposition 1 summarizes the conditions under when the price sensitivity of category A increases after assortment expansion. Throughout this section, we assume that the transportation cost s_0 is relatively small such that the benefit of one-stop shopping is never large enough to move the customer from one side to the other side of point O on the demand curve. In this two category example, we require $s_0 < |Q_A^{-1}(\frac{1}{2})|$.

Proposition 1. *The price sensitivity of category A increases after assortment expansion if $v_A < \bar{v}_A = \log \left(\frac{\exp(v_B)}{\exp(v_B + \Gamma_{AB} - s_0) + 1} \right)$.*

Proof. See Appendix.

Note also that Proposition 1 can be equivalently stated as the price sensitivity of category A increases after assortment expansion if $\Gamma_{AB} < \bar{\Gamma}_{AB} = \log \left(\frac{1 + \exp(v_B) - \exp(v_A)}{\exp(v_A + v_B)} \right) + s_0$. Figure 2 demonstrates whether the price sensitivity of category A increases in different parameter regions of v_A and Γ_{AB} . The price sensitivity of the existing category increases if v_a and Γ_{AB} are both small.

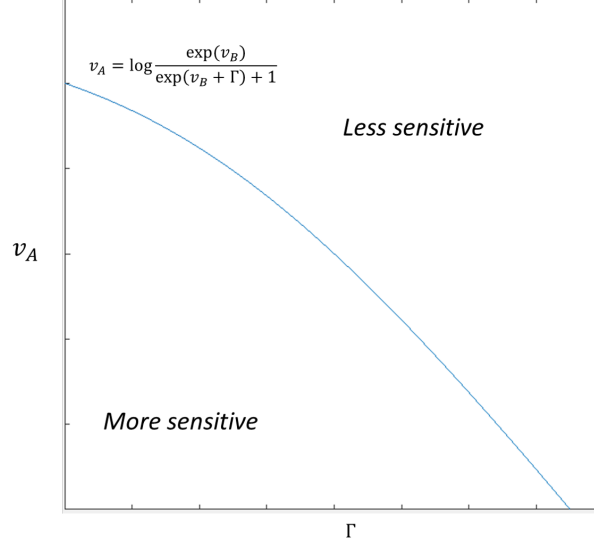


Figure 2: Change in Price Sensitivity after Assortment Expansion

2.2.2. More than two categories

We now move to the case where there are more than two categories. We assume that in addition to category A , store G also carries category C before assortment expansion. And the customer's preference is given by

$$\begin{aligned}
 u_0 &= \epsilon_0 \\
 u_A &= \alpha_A - \beta p_A + \epsilon_A \\
 u_B &= \alpha_B - \beta p_B + \epsilon_B \\
 u_C &= \alpha_C - \beta p_C + \epsilon_C \\
 u_{AB} &= \alpha_A + \alpha_B - \beta(p_A + p_B) + \Gamma_{AB} - s + \epsilon_{AB} \\
 u_{BC} &= \alpha_B + \alpha_C - \beta(p_B + p_C) + \Gamma_{BC} - s + \epsilon_{BC} \\
 u_{AC} &= \alpha_A + \alpha_C - \beta(p_A + p_C) + \Gamma_{AC} + \epsilon_{AC} \\
 u_{ABC} &= \alpha_A + \alpha_B + \alpha_C - \beta(p_A + p_B + p_C) + \Gamma_{ABC} - s + \epsilon_{ABC}
 \end{aligned} \tag{2.2}$$

We first analyze the change in the level of demand post assortment expansion. Recall that $Q_c(s)$ denotes the total demand for category c ($c = A, B, C$). With a slight abuse of

notation, let $Q_b(s)$ denote the total demand for bundle b , e.g., $Q_{AB}(s) = D_{AB}(s) + D_{ABC}(s)$.

The following lemma summarizes our findings.

Lemma 1. *The demand for category A increases after assortment expansion if $\Gamma_{AB} > \bar{\Gamma}_{AB}$ where $\bar{\Gamma}_{AB}$ solves*

$$Q_{AB}(s_0) = \frac{Q_A(s_0)}{1 - Q_A(s_0)} (Q_{BC}(s_0) - Q_{ABC}(s_0)).$$

Proof. See Appendix.

Unlike the previous case, when there are three categories, while the customer no longer pays the transportation cost after assortment expansion, her demand for the existing category A does not necessarily increase. There are two competing effects in play. First, because there is no transportation cost, the customer is more likely to purchase A together with the new category B . At the same time, the customer is also more likely to purchase the other existing category C with B , leading to cannibalization of A . Only when the complementarity between A and B is larger than the complementarity between C and B , the customer's demand for category A increases. For instance, after liquor was introduced to the grocery stores, the customer may want to pair cheese with liquor instead of wine and the demand for wine may decrease. Only when the complementarity between A and B is strong will the demand for category A increase.

Since the purchase probability for category A may either increase or decrease depending on the degree of complementarity between A and B , the changes in price sensitivity can go either way, too. The following proposition summarizes the conditions under which the price sensitivity of existing categories increases after assortment expansion.

Proposition 2. *After assortment expansion, the price sensitivity of category A increases when (v_A, Γ_{AB}) satisfies*

1. $v_A > \bar{v}_A(\Gamma_{AB})$ and $\Gamma_{AB} < \bar{\Gamma}_{AB}(v_A)$, or

2. $v_A < \bar{v}_A(\Gamma_{AB})$ and $\Gamma_{AB} > \bar{\Gamma}_{AB}(v_A)$.

The price sensitivity of category A decreases when

1. $v_A > \bar{v}_A(\Gamma_{AB})$ and $\Gamma_{AB} > \bar{\Gamma}_{AB}(v_A)$, or
2. $v_A < \bar{v}_A(\Gamma_{AB})$ and $\Gamma_{AB} < \bar{\Gamma}_{AB}(v_A)$.

Here \bar{v}_A solves $Q_A(v_A) = \frac{1}{2}$. $\bar{\Gamma}_{AB}$ solves $Q_{AB}(\Gamma_{AB}) = \frac{Q_A(\Gamma_{AB})}{1-Q_A(\Gamma_{AB})} (Q_{BC}(\Gamma_{AB}) - Q_{ABC}(\Gamma_{AB}))$.

Proof. See Appendix.

Table 1 summarizes the predictions in Proposition 2. For each customer, the existing product categories can be classified into four groups depending on their baseline preference and bundle utility. We expect price sensitivity to increase in categories for which the customer has a weak preference and are strong complements for the new category and those with strong preference and are substitutes for the new category.

Table 1: Changes in Price Sensitivity in Different Parameter Regions

	$\Gamma_{AB} < \bar{\Gamma}_{AB}$	$\Gamma_{AB} > \bar{\Gamma}_{AB}$
$v_A > \bar{v}_A$	increase	decrease
$v_A < \bar{v}_A$	decrease	increase

2.3. Empirical Context and Data

The stylized model presented in Section 2.2 suggests that price sensitivity may increase after assortment expansion due to the (decreased) transportation cost. In addition, this effect is heterogeneous across customers and categories depending on their characteristics. In this section, we examine this phenomenon in an empirical setting.

2.3.1. Empirical context

A natural concern when identifying the causal impact of assortment expansion on price sensitivity is endogeneity. Retailers often strategically tailor their assortments to demand shocks, which may not be observed by the researchers. If these demand shocks are also

correlated with price sensitivity (e.g., seasonal demand), we may falsely conclude that price sensitivity changes due to assortment expansion. Hence, we would ideally require the expansion to be independent of temporary demand shocks.

Our empirical analyses leverage a natural experiment induced by the state of Washington’s deregulation of liquor markets. Before June 2012, the state of Washington held a monopoly on liquor sales through a state-owned chain, “Liquor and Wine”. From June 1, 2012 onward, private retailers with premises larger than 10,000 square feet were allowed to enter the market. This state-wide policy demonstrates several desirable properties for our analyses. First, while the policy change is anticipated, prior research on the Washington State liquor market found that it is unlikely for retailers to game the footage restriction (Illanes and Moshary, 2020). Furthermore, among the stores that partner with Nielsen, 99% opted to carry liquor immediately after June 1, 2012. This indicates that conditional on the store type (chain stores), the square footage requirement is actually not binding, which alleviates the concern for self-selection. For these reasons, we treat the assortment expansion as exogenous.⁵ The empirical context thus provides an appealing setting for investigating the (causal) impact of assortment expansion on price sensitivity.

2.3.2. Data

Our primary data source is the Nielsen Consumer Panel for the year 2012. The data consists of all transactions by a representative panel of 1,500 households in the state of Washington. The data contains information on the specific store visited, visit date, products purchased, quantity purchased, and the price paid. We also have a few demographics of the households, including their annual income and the zip code they reside in. We combine information on the location of the state-owned liquor stores from WSLCB and calculate the density of liquor stores prior to privatization in each. We supplement this data with two other sources of data that helps to construct the prices of all the products in the customers’ choice sets. First,

⁵In Appendix A.4, we validate this assumption by showing that stores carrying liquor after June 2012 are comparable to those without liquor. In particular, we compare the treated stores in Washington with Oregon stores in the same chains in terms of their sales before June 2012. We find these stores have statistically comparable time trends.

we use Nielsen Retailer Scanner Data that includes the average weekly prices, quantity, and product information for all products sold in stores that partner with Nielsen, including liquor that appear in grocery stores after assortment expansion. To construct liquor prices in periods before June 2012, we obtain data from WSLCB that tracks the sales and prices of liquor in each state-owned liquor store. In some of the descriptive analyses where we will compare the State of Washington to the State of Oregon, we supplement the main dataset with the same Nielsen data for Oregon.

2.3.3. Sample selection and construction

To understand the impact of assortment changes on price sensitivity, we have to balance more granular data (e.g. at the UPC level which would potentially number in the thousands) with model and empirical parsimony. Therefore, to reduce dimensionality and alleviate the computational burden, we make the following choices when specifying the unit of analysis. First, we model purchase at the category level that corresponds to Nielsen’s definition of product department. Specifically, we examine purchase from 7 food categories: dry grocery (e.g., canned fruit, juice), frozen foods (e.g., frozen vegetables and entrees), dairy (e.g., milk, butter), deli (e.g., prepared salad and sandwiches), packaged meat (e.g. ham, bacon), alcoholic beverages (e.g., beer, wine), and liquor.⁶ Second, we adopt a week as the period when the consumer plans her shopping and aggregate the purchases to the weekly level.⁷

We also restrict our attention to a subset of households and stores to reduce the complexity of the analysis. First, we focus on state-owned liquor stores and grocery stores partnering with Nielsen in the Retail Scanner Data for which the price information is available. Customer trips to these stores contain 62.7% of the shopping trips in the data. Second, we restrict our attention to frequent customers who have at least 5 purchases during the period and have at least one liquor purchase. Finally, to ensure that the estimated overall change

⁶Liquor (defined by WSLCB as spirits and hence regulated) and other alcoholic drinks are grouped together under the alcoholic beverage department in the Nielsen data. For our analysis, we define them as two distinct categories, liquor and (other) alcoholic beverages.

⁷We generate qualitatively similar results using a day as the shopping period. These results are available upon request.

in the demand sensitivity is driven by changes in individual behavior, rather than changes in the composition of customers, we also require customers to have at least one purchase at the stores before the introduction of liquor so that we have a panel data of customer purchase.⁸ The final data set we analyze therefore consists of 11,305 shopping trips made by 221 households.

We next explain how we aggregate the data to the category level given that the Retail Scanner Data provides price and sales information at the UPC level. To obtain category level prices, we follow previous research (e.g. Manchanda, Ansari, and Gupta, 1999) and compute the sales-weighted price of UPCs within each category for each store-week combination. We first normalize all prices to unit prices where the units are chosen to be the modal size in each category (e.g. price per 16 oz). Then we average the price across UPCs using sales volumes. The weights are specified as the average weekly store-level UPC sales in 2012, which is fixed over time to ensure the changes in prices reflect the changes in UPC prices rather than substitutions between UPCs within a category. Table 2 presents the summary statistics of the data.

Table 2: Summary Statistics

<i>Panel A. Purchase Behavior (N=11,305)</i>		
Category	Tripwise Purchase Probability	
Liquor	0.066	
Dry grocery	0.779	
Frozen foods	0.324	
Dairy	0.461	
Deli	0.164	
Packaged meat	0.143	
Alcoholic beverages	0.115	
<i>Panel B. Customer Characteristics (N=221)</i>		
	Mean	SD
Annual Income (\$)	57,131	28,990
Liquor Store Density (/mi ²)	0.055	0.157

⁸Changes in customer composition due to category expansion are also of general interest. We briefly discuss it in our future research section.

2.3.4. Descriptive evidence

In this section, we provide descriptive evidence suggesting that customers value the convenience of one-stop shopping and that they are more sensitive to price changes when purchasing other categories after assortment expansion. To do that, we begin by documenting that customers are more likely to purchase groceries and liquor within the same shopping trip after liquor appears in grocery stores, suggesting customers prefer the convenience of one-stop shopping. We then show that price sensitivity for existing food categories increases and subsequent optimal prices.

Preference for One-stop Shopping

Table 3 reports the percentage of shopping trips that include liquor and/or existing grocery categories before and after June 2012. There are several notable observations. First, customers prefer to co-purchase liquor with groceries within one trip. After liquor appears in grocery stores, the conditional purchase probability of liquor (conditional on purchasing groceries) is $Pr(\text{Liquor} | \text{Grocery}) = \frac{Pr(\text{Liquor}, \text{Grocery})}{Pr(\text{Grocery})} = 9.4\%$, which is larger than the marginal purchase probability of liquor (7.8%), suggesting that liquor may well likely be a complement to groceries. Second, liquor is more likely to be purchased with groceries after they appear in the same stores. Specifically, the probability of co-purchasing liquor and groceries increases from 3.9% to 7.5%. This suggests that customers get more utility from co-purchase after assortment expansion, i.e., they value the convenience of one-stop shopping.

Table 3: Cross Tabulation of Grocery and Liquor Purchases

	Before			After	
Pr(A,B)	No grocery	Grocery		No grocery	Grocery
No liquor	12.5%	82.6%	No liquor	12.3%	79.9%
Liquor	1.0%	3.9%	Liquor	0.3%	7.5%

Changes in when, what, and how much to purchase

We now turn to investigate how the introduction of liquor affects customers' purchases in existing categories. We are interested in how the introduction of liquor change purchase incidence, purchase quantity, and total spending. We show that liquor sales increases. This is mainly driven by the increase in purchase incidence, which becomes the primary focus of this research.

To examine the effect of the new category' introduction on purchase behavior in the existing categories, we complement our main data set with shopping records from Oregon customers and consider a difference-in-differences model (Angrist and Pischke, 2008). We choose Oregon customers as controls because in the state of Oregon, liquor can only be purchased at state-run liquor stores throughout 2012. Equation (2.3) estimates the changes in Washington customer's behavior after the grocery stores started to carry liquor, compared to the Oregon customers who faced no change in assortments. Let i denote a customer and t denote a week. We estimate the following model:

$$y_{ict} = \beta \text{Treated}_i \cdot \text{After}_t + \alpha_{ic} + \gamma_{ct} + \epsilon_{it} \quad (2.3)$$

where y_{ict} is an outcome of interest for customer i and category c in week t . We use three outcome variables, namely total spending, purchase incidence, and number of units conditional on making a purchase. The first outcome variable helps us to understand the overall impact of the new category on revenue from the existing categories. The latter two outcome variables further attribute the effect into when, what, and how much customers purchase. Treated_i is an indicator variable that equals 1 if i is a Washington customer, After_t is an indicator variable that equals 1 if t is after June 2012, α_{ic} are individual-category fixed effects that capture customers' time-invariant tastes, and γ_{ct} are week fixed effects that capture common time trends across customers due to, for example, seasonality.

The results from Equation (2.3) are reported in Table 4. Column (1) shows that Washington

customers' total spending in other categories increases, compared to the Oregon customers. Columns (2) and (3) further demonstrate the source of this increase. Specifically, our results indicate that there is no significant increase in purchase quantity. However, we see that the purchase probability of other categories increases by 0.8%. This suggests that our focus in this research on category incidence may be well suited for our empirical setting studied here. However, we discuss in Section 2.6 how an integrated model where assortment expansion influences incidence, and quantity could be of significant empirical interest. For the rest of the paper, we focus on the purchase incidence of a category as the main outcome of interest.

Table 4: Changes in Purchase Behavior after Assortment Expansion

	Dependent variable		
	Total Spending (1)	Purchase incidence (2)	Purchase quantity (3)
Treatment effect (β)	1.351*** (0.421)	0.008*** (0.003)	0.032 (0.065)
Observations	110,424	110,424	40,913
Adjusted R-squared:	0.293	0.117	0.256

Note: *** p<0.01 ** p<0.05 *p<0.1
Robust standard errors in parentheses

Descriptive evidence of changes in price sensitivity

We now move to this research's central question: whether there is evidence of a change in price sensitivity and how the changes vary across categories and customers. We estimate the change in price sensitivity using a difference-in-differences specification, again using Oregon customers as controls. Let i denote a customer, t denote a week, and c denote a category. We estimate the following model:

$$\begin{aligned}
y_{ict} = & \beta \text{Treated}_i \cdot \text{After}_t + \tau_1 \text{Price}_{ict} + \tau_2 \text{Price}_{ict} \cdot \text{Treated}_i + \tau_3 \text{Price}_{ict} \cdot \text{After}_t \\
& + \tau_4 \text{Price}_{ict} \cdot \text{Treated}_i \cdot \text{After}_t + \tau_5 X_{ic} \cdot \text{Price}_{ict} \cdot \text{Treated}_i \cdot \text{After}_t + \alpha_{ic} + \gamma_{ct} + \epsilon_{ict},
\end{aligned}
\tag{2.4}$$

where y_{ict} and Price_{ict} are the purchase incidence and price of existing categories, respectively. Treated_i is an indicator variable that equals one if i is a Washington customer and zero otherwise, After_t is an indicator variable that takes 1 after June 2012 and zero otherwise, α_{ic} are individual-category fixed effects, and γ_{ct} are category-week fixed effects. We also allow the treatment effect to be heterogeneous across customers and categories depending on their characteristics X_{ic} by including a higher-order interaction term. Recall that in Section 2, we show that price sensitivity increases when 1) customer’s preference for the category is relatively weak, and the focal category is a relatively strong complement to liquor, or 2) customer’s preference for the category is relatively strong and the category is a substitute to liquor. Motivated by these predictions, X_{ic} includes the proxy for preference for the category (the probability of purchasing category c) and the proxy for complementarity between the category and liquor (probability of co-purchasing category c with liquor before June 2012). The key parameter of interest is the change in price sensitivity, τ_4 . We are also interested in τ_5 , which helps understand how the effect varies across customers and categories. Table 5 presents the results from Model (2.4). We find τ_4 to be significantly negative at -0.017 ($p < 0.05$). Compared to the baseline price sensitivity, customers are on average 9 % more price sensitive for existing categories after the introduction of liquor. In addition, the change in price sensitivity varies across categories. The higher-order interaction with purchase frequency is positive and significant. Consistent with our theory, customers are more price-sensitive in categories with low purchase probability and less price-sensitive in categories with high initial purchase probability.

Alternative explanations

So far we have shown that price sensitivity for other food categories may increase after the entry of liquor. In this section, we discuss several alternative explanations for the descriptive evidence that motivate the use of a formal demand model.

First, it is important to consider whether other elements of the marketing mix change after the introduction of the liquor category. For example, a store’s product assortments and

Table 5: Regression Results

	Dependent variable	
	Purchase Incidence	
	(1)	(2)
Treatment * After (β)	0.030*** (0.010)	0.035*** (0.009)
Price (τ_1)	-0.196*** (0.006)	-0.197*** (0.006)
Price * Treatment (τ_2)	-0.016*** (0.006)	-0.016** (0.006)
Price * After (τ_3)	0.010 (0.007)	0.011 (0.007)
Price * Treatment * After (τ_4)	-0.017** (0.008)	-0.017** (0.008)
Price * Treatment * After * Frequency (τ_5)		0.018*** (0.0002)
Price * Treatment * After * Co-purchase (τ_6)		0.001** (0.001)
Store-category fixed effect	Y	Y
Week-category fixed effect	Y	Y
Observations	110,424	110,424
Adjusted R ²	0.184	0.227

Note: *** p<0.01 ** p<0.05 *p<0.1. Robust standard errors in parenthesis.

product quality in the existing categories may change due to the shrinking shelf space. Similarly, a store may also adjust its promotional strategy after the expansion. If prices are positively correlated with these characteristics that are not included in the model, the estimated price sensitivity will be biased, and we may falsely conclude that customers become more price sensitive.

We evaluate the changes in the stores' other marketing mix using a difference-in-differences model

$$w_{sct} = \beta \text{Treated}_s \cdot \text{After}_t + \alpha_{sc} + \gamma_{ct} + \epsilon_{sct} \quad (2.5)$$

where w_{sct} is a marketing mix variable for store s in week t . We use four outcome variables, namely price, assortment size (measured by the number of unique UPC offered), display intensity (measured by % UPCs on display), and advertising intensity (measured by % UPC advertised). Treated_s is an indicator variable that equals 1 if store s is a Washington store, After_t is an indicator variable that equals 1 if t is after June 2012, α_{sc} is store-category fixed effects, and γ_{ct} is category-week fixed effects. Table 6 presents the results. We find that assortment expansion is indeed associated with changes in other marketing mixes. Particularly, after the stores started to carry liquor, their assortments and feature advertising in the other categories decrease. This is in line with the fact the grocery stores have fixed shelf space, and the introduction of a new category may be at the expense of existing categories. The identification of price sensitivity thus requires a model that controls for other changes in the market.

In addition, while complementarity is not our primary focus, it is also important to measure complementarity accurately for two reasons. First, our predictions are based on the primitive that customers value one-stop shopping. To pin down the transportation cost empirically, we have to accurately measure the degree of complementarity both before and after assortment expansion. Also, recall that our analytical model predicts that the magnitude and sign of the change in price sensitivity depend on complementarity. An unbiased estimate of complementarity also allows us to test the theoretical predictions. Gentzkow (2007) finds

Table 6: Changes in Marketing Mix after Assortment Expansion

	Dependent variable			
	Price (1)	Assortment (2)	Display (3)	Feature (4)
Treatment effect (β)	0.006*** (0.0002)	-0.175** (0.064)	-0.0002 (0.0002)	-0.0007*** (0.0003)
Observations	167856	167856	167856	167856
Adjusted R-squared:	0.020	0.034	0.356	0.211

Note: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

that category complementarity can be confounded with correlation as both could result in highly correlated purchases across categories. Hence, to conclusively pinpoint that liquor complements other categories, we must account for the unobserved correlation in consumer preferences.

2.4. Demand Model

The descriptive analyses reveal that the existing categories are on average complements to the new category (liquor) and that the customers are more price-sensitive for these categories post assortment expansion. However, as discussed in Section 2.3, there are several potential confounders. In this section, we study the effect of assortment expansion on price sensitivity using a formal demand model. We build on the analytical model in Section 2.2 and extend the model to fit our empirical setting. Specifically, we model customer multi-category purchase in a bundle utility framework that incorporates co-purchase of multiple product categories, correlated preferences, price endogeneity, as well as heterogeneity across customers. The results of the demand model allow us to explore the change in price sensitivity for each of the six existing categories after assortment expansion through counterfactual simulations. We also quantify the potential monetary loss if retailers do not consider these changes when setting prices.

2.4.1. Model setup

Our demand model characterizes customers' purchases from seven food categories under a bundle utility framework (Gentzkow, 2007). In each shopping trip, a customer chooses among all possible bundles (combinations) of categories and picks the one that gives her the highest utility. However, the utility of a bundle may be larger (smaller) than the sum of utility from the individual categories when they are complements (substitutes). In what follows, we first specify the utility for each category and describe the bundle utility framework. We then discuss confounders that may bias the estimation of the degree of complementarity and thus the transportation cost, e.g., correlated preferences and price endogeneity, and show how we account for these confounders. We conclude with the full model specification for estimation.

Let $i \in \{1, \dots, I\}$ index consumers, $t \in \{1, \dots, T_{(i)}\}$ index shopping occasions⁹, $c \in \{1, \dots, C\}$ index categories, and $b \in \{1, \dots, B\}$ index possible product bundles, including the singleton bundles.

Base utility of a single category

We define the base utility v_{ict} of a (single) category c in shopping trip t by consumer i to be

$$v_{ict} = \alpha_{ic} + \sum_{m=2}^{12} \eta_{cm} \mathbf{I}(t \in m) + \xi_{ict} + \beta_i p_{ict}. \quad (2.6)$$

Here, α_{ic} is the baseline preference for each category. In general, $\text{cov}(\alpha_{ic}, \alpha_{ic'}) \neq 0$ so that the preference for the categories may be correlated, η_{cm} are category-month fixed effects that measure the deviations of preferences in month m relative to the first month due to seasonality. For instance, customers may have stronger preferences for alcoholic beverages during holiday seasons. These fixed effects also pick up any systematic changes in the marketing mix after the introduction of liquor, including changes in category quality,

⁹The shopping occasions are individual-specific. We suppress the subscripts for exposition purposes hereafter.

assortments, and feature advertising. β_i is the heterogeneous price coefficient, p_{ict} is the price of the product category c , and ξ_{ict} is unobserved category characteristics or demand shocks that may be correlated with prices. We address the price endogeneity through a control function approach (Petrin and Train, 2010), which we will describe later.

Bundle utility and transportation cost

After defining the utility of individual product categories v_{ict} , we now specify the utility of a bundle. Customer i 's utility from purchasing bundle b in shopping trip t is

$$u_{ibt} = \begin{cases} \sum_{c \in b} v_{ict} + \Gamma_{ib} - s_i \cdot I(t \in T^{before}) \cdot I(\text{liquor} \in b) + \epsilon_{ibt}, & \text{when } |b| \geq 2; \\ v_{ibt} + \epsilon_{ibt}, & \text{when } |b| = 1; \\ \epsilon_{ibt}, & \text{when } |b| = 0. \end{cases} \quad (2.7)$$

Here Γ_{ib} is an interaction that determines how the utility from the bundle b differs from the sum of the underlying utilities from categories $c \in b$. Note that without some form of restriction there would be $2^7 - 7 - 1 = 120$ Γ_{ib} terms in the model. For the model to be tractable, following Dubé (2019), we assume that¹⁰

$$\Gamma_{ib} = \sum_{c \in b} \sum_{c': c' \in b, c' < c} \Gamma_{i\{c, c'\}}. \quad (2.8)$$

s_i is the transportation cost incurred by consumer i when she visits both grocery store and liquor store within the same trip, $I(t \in T^{before})$ is an indicator variable that equals 1 when the shopping trip t occurs before the privatization of liquor and zero otherwise, and $I(\text{liquor} \in b)$ is an indicator variable that equals 1 when bundle b includes both liquor and grocery items. The customer pays the transportation cost when she visits two stores to purchase liquor and grocery within a single shopping trip before the privatization of liquor.

¹⁰It can be shown that under this assumption, the bundle utility maps into cross-price effects. To illustrate, consider a three-category case. When $\Gamma_{123} = \Gamma_{12} + \Gamma_{13} + \Gamma_{23}$, the complementarity between category 1 and 2 will be determined by a direct effect Γ_{12} and an indirect effect $\Gamma_{13}\Gamma_{23}$ whose sign is the same as $\Gamma_{13}\Gamma_{23}$. In Section 5, we revisit this assumption and show its validity by demonstrating that this model predicts co-purchase well.

ϵ_{ibt} is the idiosyncratic shock for each bundle that captures all remaining variation and follows i.i.d. type-I extreme value distribution.

Price endogeneity

As discussed in Section 2.4.4, one typical concern in demand estimation is price endogeneity. For example, a store’s product assortments and product quality in the existing categories may change due to the shrinking shelf space after the introduction of liquor. Similarly, a store may also adjust its promotion strategy after the expansion. If prices are positively correlated with these characteristics not included in the model, the estimated price sensitivity will be biased. While we have included flexible fixed effects, they may not fully capture the unobserved product characteristics or demand shocks. To address this issue, we use a control function approach following Petrin and Train (2010).

Specifically, we first regress price p_{ct} onto a set of exogenous instruments Z_{ct} ,

$$p_{ict} = \tau_c' Z_{ict} + e_{ict}. \quad (2.9)$$

We use Hausman-type instruments, i.e., average prices in other stores in the state of Washington in the same time period (Hausman, 1996; Nevo, 2001). The assumption is that prices across different stores are affected by common cost shifters and the unobserved demand shocks are independent across stores (conditional on other observables). The residual of this regression \hat{e}_{ict} is the price shock that may capture the unobserved demand shocks, product characteristics, and advertising activities. These residuals are retained and are used to form the control function. We specify the control function as a linear function of the unobserved shock $\hat{\xi}_{ict} = \phi \hat{e}_{ict}$. The control function then enters the category utility (Equation (2.6)) as an extra variable and replaces ξ_{ict} .

Final likelihood

Taken together, consumer i 's utility of purchasing bundle b in shopping trip t is

$$u_{ibt} = \sum_{c \in b} \left[\alpha_{ic} + \sum_{m=2}^{12} \eta_{cm} \mathbf{I}(t \in m) + \xi_{ict} + \beta_i p_{ict} \right] + \sum_{c \in b} \sum_{c': c' \in b, c' < c} \Gamma_{i\{c, c'\}} - s_i \cdot \mathbf{I}(t \in T^{before}) \cdot \mathbf{I}(\text{liquor} \in b) + \epsilon_{ibt}. \quad (2.10)$$

The probability of purchasing bundle b is then

$$\text{Prob}(y_{it} = b) = \frac{\exp(u_{ibt})}{1 + \sum_{b' \in B} \exp(u_{ib't})}. \quad (2.11)$$

The likelihood of the observed data is given by

$$L(\Theta) = \prod_i \prod_t \prod_b \text{Prob}(y_{it} = b)^{1(y_{it}=b)} \cdot [1 - \text{Prob}(y_{it} = b)]^{[1-1(y_{it}=b)]}. \quad (2.12)$$

Heterogeneity

We allow for heterogeneity across customers using a hierarchical Bayes specification. A hierarchical Bayes model is well suited for studying multi-category purchases as it naturally allows for correlated individual-level parameters. It also leverages data on rare co-purchase behavior across customers to make inferences at the individual level.

We assume that the customers are heterogeneous in their baseline preferences for the individual product categories, price coefficients, bundle utility, and transportation cost. Furthermore, we allow the individual-level parameters to be dependent on customer i 's observed characteristics. Specifically, we assume

$$\Theta_i = (\alpha_{ic}, \beta_i, s_i) \sim \text{MVN}(\Theta_0 + \Theta_1 X_i, \Sigma_\Theta). \quad (2.13)$$

where X_i is a vector of individual characteristics that include (standardized) family income

and liquor store density in the (5 digits) zip code of residence before the privatization of liquor. We hypothesize that the transportation cost is negatively correlated with store density. In addition, customers may have different price coefficients (marginal utility of income) depending on their income levels. We note that Σ_{Θ} is a non-diagonal matrix and hence inherently allows for correlation in the unobserved taste shocks across products. Finally, we assume the bundle utilities follow independent normal distributions, i.e.,

$$\Gamma_{ib} \sim N(\Gamma_{0b}, \sigma_{\Gamma_b}^2) \quad (2.14)$$

for each two-category bundle b . We note that while Γ_{ib} is assumed to be independent, the utilities of different bundles can be correlated for two reasons. If two bundles share common categories, their utilities are correlated through the common categories in the bundles. Even though there is no common category, the utilities of two bundles may be correlated due to the correlation in the base utility of each category.

2.4.2. Identification and estimation

We briefly describe the identification of the bundle utility, and the transportation cost. The identification of other parameters (e.g., baseline utility and monthly effects) follows standard arguments. First, the bundle utility Γ and the correlation in preference (off-diagonal elements of Σ_{Θ}) jointly determines the co-purchase behavior post assortment expansion. If the joint purchase probability is high relative to the marginal purchase probabilities, either the bundle utility or the correlation in preference is high. Separate identification of these two parameters is based on the panel data structure. For example, if the observed frequent co-purchase is explained by the correlation in purchase across time rather than correlation across customers, i.e., a given customer purchases both categories in one shopping trip and neither in another shopping trip, this implies a positive and large Γ . On the other hand, if the co-purchase behavior is explained by the correlation in purchase across customers, i.e., some customers purchase both but others purchase neither, this suggests a large correlation in preference. After Γ and Σ_{Θ} are identified, the transportation cost s is identified by the

changes in co-purchase behavior before and after assortment expansion. An increase in co-purchase probability indicates a positive s .

We estimate the model using a Hamiltonian Monte Carlo (HMC) algorithm within Stan (Carpenter et al., 2017). See Appendix A.5 for our choice of priors and hyperpriors, as well as the detailed estimation procedure. To verify that our model is empirically identified, we simulate data for the same number of individuals and shopping trips using parameter values similar to those in our observed data. We then estimate the model for this simulated data. We find that the estimates are reasonably close to the true values and the estimated 95% credible intervals contain the true means for each parameter. We conclude that the model can be identified and estimated well given the size and variation in the observed data. See Appendix A.6 for results of the parameter recovery.

2.5. Results

In this section, we present the results from our empirical model. We first evaluate the model fit of the specified model and its nested versions. We show that our model fits the data well through both deviance information criterion (DIC) and posterior predictive checks (Gelman et al., 2013). We then report the parameter estimates. Finally, based on the parameter estimates, we examine the change in price sensitivity and comment on its economic significance.

2.5.1. Model Fit

The observed purchase behavior is jointly determined by the base utility of each category, the transportation cost, bundle utilities, and correlated preferences across customers. To examine whether these parameters contribute to explaining the purchase patterns, we examine the fit of the full model and three nested models. The first nested model assumes there is no transportation cost ($s_i = 0$). The second nested model further removes bundle utilities ($\Gamma_{ib} = 0$ for all b) but allows for correlated preferences across categories. The third model does not allow for correlated preferences (off-diagonal elements of Σ_Θ are zero). Column (1) of Table 7 reports the DIC of each model. A lower value suggests a better

model fit. The full model gives the lowest DIC and fits the data the best. Incorporating the transportation cost s , bundle utilities Γ , and correlated preferences significantly improve the model fit.

We also evaluate the model fit via posterior predictive checks. To construct the posterior predictive checks, we take 800 posterior samples of each individual and simulate category choices in each shopping trip conditional on the observed prices. We then compare the simulated purchase behavior, in particular co-purchase probability and the number of unique categories purchased, with the observed ones.

We start by examining the probability of co-purchasing liquor with groceries. The introduction of liquor in grocery stores offers customers the convenience of one-stop shopping and encourages co-purchasing liquor and groceries within the same trip. Columns (2) and (3) in Table 7 report the probability of co-purchasing liquor and grocery categories in the same trip *before and after* the introduction of liquor. The full model captures the co-purchase probability well in both periods. Consistent with the observed purchase behavior, the probability of co-purchasing liquor and grocery categories increases by 3.4% after the introduction of liquor. In contrast, models without transportation cost will underestimate this increase in co-purchase probability.

Table 7: Model Fit

	DIC	Co-purchase of liquor and groceries		Number of categories
		Before	After	
Full model	21674	3.91% ([2.82%, 5.17%])	7.31% ([5.91%, 8.70%])	3.22 ([3.20, 3.24])
No transportation cost	21948	4.52% ([3.23%, 6.16%])	7.07% ([5.83%, 8.64%])	3.23 ([3.20, 3.26])
No transportation cost and bundle utility	22059	4.65% ([3.18%, 6.07%])	7.15% ([5.73%, 8.63%])	3.22 ([3.20, 3.24])
No transportation cost, bundle utility, and correlation	22113	4.50% ([3.16%, 6.11%])	7.13% ([5.75%, 8.49%])	3.23 ([3.19, 3.27])
Observed		3.91%	7.47%	3.22

Note: 95% credible intervals in parentheses.

We also examine the number of unique categories purchased during a trip. Column (4) in Table 7 reports the predicted and observed average number of categories purchased in a trip. We find that the four models all give reasonable predictions at the aggregate level. We

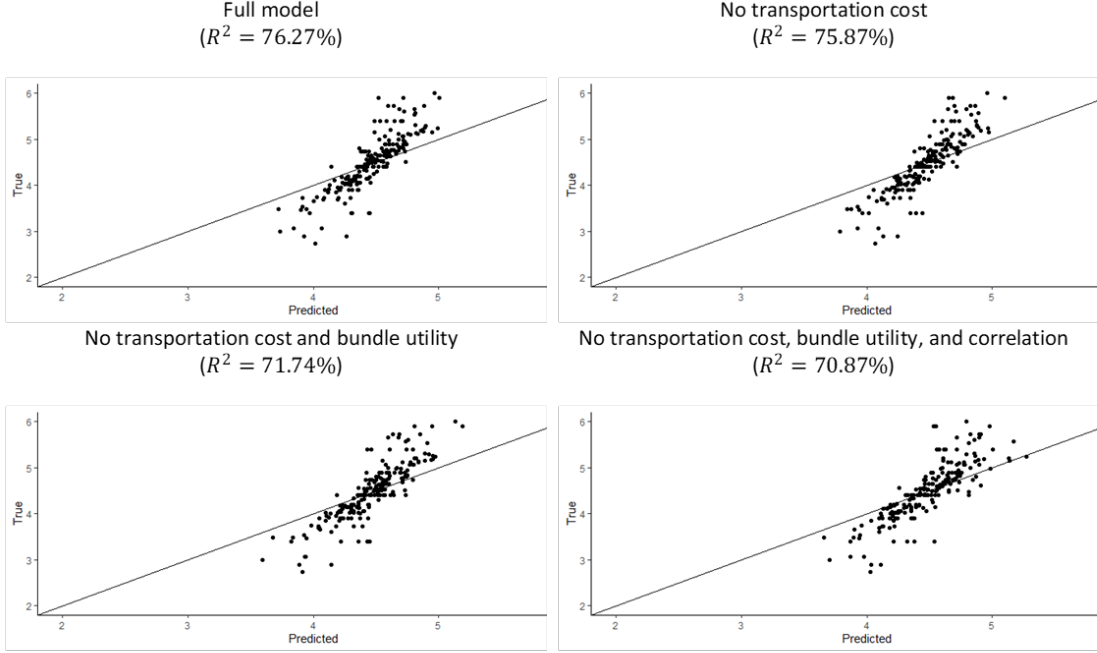


Figure 3: Predicted and Observed Individual-level Co-purchase

further assess whether these models can capture the heterogeneity in the purchase patterns. Figure 3 plots the individual-level co-purchase behavior based on the observed and simulated data. We find that the full model outperforms the nested ones at the individual level. The predicted values from the full model and the observed ones are reasonably correlated. Regressing observed values onto the predicted ones gives an R^2 of 76.27%, suggesting that the full model predicts co-purchase well. This also confirms a degree of validity of our modeling assumptions. Recall that instead of having fully parameterized bundle utilities, we make a simplifying assumption that the bundle utility of bundles with more than two categories is simply the sum of all possible pairwise bundle utilities. Since our model can predict the number of categories purchased within a trip well, this ensures that the model can correctly capture the substitution patterns across categories.

2.5.2. Model Estimates

Table 8 presents the parameter estimates from our empirical model. The first two columns report the posterior means and the 95% credible intervals of the population-level param-

ters Θ_0 and Γ_0 , which includes the category intercepts for 7 product categories, the price coefficients, the transportation cost, and the bundle utility between liquor and the six existing categories. Column 3 through 6 shows the posterior means and the 95% intervals of Θ_1 , demonstrating the observed heterogeneity depending on customer characteristics.

Table 8: Model Estimates

	Main effect		Household income		Liquor store density	
	Mean	95% CI	Mean	95% CI	Mean	95% CI
Price coefficient (β)	-0.53	[-0.70, -0.34]	-0.03	[-0.12, 0.07]	-0.02	[-0.11, 0.10]
Transportation cost (s)	1.92	[0.81, 3.03]	0.27	[-0.19, 0.75]	-0.28	[-0.59, 0.02]
Category intercept (α): Liquor	-0.70	[-1.89, 0.28]	0.10	[-0.23, 0.37]	0.15	[-0.18, 0.52]
Dry grocery	1.87	[1.32, 2.44]	0.05	[-0.20, 0.27]	-0.18	[-0.40, 0.07]
Frozen foods	-1.01	[-1.51, -0.47]	0.01	[-0.13, 0.17]	0.02	[-0.14, 0.20]
Dairy	-0.81	[-1.21, -0.47]	-0.04	[-0.17, 0.09]	-0.03	[-0.18, 0.12]
Deli	-1.17	[-1.84, -0.55]	0.14	[-0.11, 0.33]	0.21	[-0.08, 0.50]
Packaged meat	-2.96	[-3.78, -2.15]	0.17	[0.00, 0.36]	0.06	[-0.15, 0.28]
Alcoholic beverages	-1.29	[-2.15, -0.47]	0.14	[-0.16, 0.43]	0.05	[-0.35, 0.36]
Bundle utility (Γ): Dry grocery	0.41	[0.24, 0.65]				
Frozen foods	0.26	[-0.29, 0.71]				
Dairy	-0.25	[-0.65, 0.12]				
Deli	-0.74	[-1.56, 0.03]				
Packaged meat	-1.30	[-2.06, -0.54]				
Alcoholic beverages	-0.18	[-0.89, 0.59]				

We find that the transportation cost s is positive and significant. This result suggests that customers value the convenience of one-stop shopping ($s_0 = 1.92$, $p < 0.01$). We calculate each customer's willingness to pay for one-stop shopping by translating the parameter value in utility space into monetary value, i.e., $-\frac{s_i}{\beta_i}$. The average willingness to pay for one-stop shopping is \$5.60. This is consistent with the literature on shopping costs. For example, Seo (2019) finds that a customer is willing to pay \$1.65 to reduce the trip distance by one mile and \$1.32 to reduce the number of stops within a trip. Thomassen et al. (2017) report the willingness to pay for one-stop shopping to be £4.98 in Britain. In addition, the transportation cost is heterogeneous across customers. Figure 4 presents the distribution of the posterior means of the individual level transportation cost, measured in dollar value. More than 95% of customers have a positive transportation cost. We also find that a customer's transportation cost is correlated with her observed characteristics. Consistent with expectation, customers who live in an area with fewer liquor stores have higher transportation

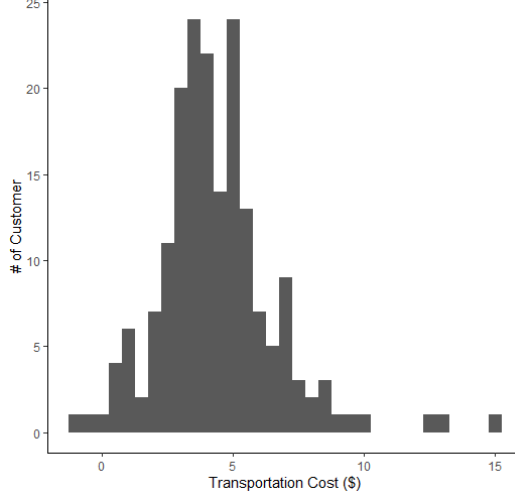


Figure 4: Distribution of Transportation Costs

costs ($s_1^{density} = -0.28$, $p < 0.01$). Also, customers with higher family income have larger transportation costs ($s_1^{income} = 0.27$, $p < 0.05$), potentially due to the higher opportunity cost of time.

We also identify the complements and substitutes of liquor by examining the bundle utility Γ . The bundle utility of liquor ($\Gamma_{dry\ grocery, liquor} = 0.41$, $p < 0.01$) and dry grocery and frozen food ($\Gamma_{frozen\ food, liquor} = 0.26$) are positive, meaning that consumers prefer co-purchasing liquor and these product categories. Among them, the dry grocery category seems to be the strongest complement. Dairy, packages meat, and alcoholic beverages are found to be substitutes for liquor. To assess the face validity of these results, we also compare the complements identified by our model with those suggested by the standard definition of complements and substitutes. By definition, when the cross-price sensitivity is negative (positive), the two categories are complements (substitutes). We calculate the cross-price sensitivity between liquor and other categories by estimating a simple linear regression using Nielsen Scanner data:

$$y_{st} = \beta_1 p_{s1t} + \dots + \beta_7 p_{s7t} + \alpha_s + \gamma_t + \epsilon_{st}, \quad (2.15)$$

where y_{st} is store s 's demand for liquor in week t , p_{s1t}, \dots, p_{s7t} are prices of liquor and other categories, α_s are store fixed effects, and γ_t are week fixed effects. The parameters β_1, \dots, β_7 thus estimate own- and cross-price sensitivities. Table 9 presents the bundle utility and the cross-price sensitivity between liquor and six existing categories. We find that in *all* of the six categories, the two methods generate directionally consistent results (whether two categories are complements or substitutes). The two methods consistently predict that liquor is a complements for dry grocery and frozen food and a substitute for dairy, deli, packaged meat, and alcoholic beverages. We conclude that our empirical model generates reasonable estimates of the bundle utility.

Table 9: Bundle Utility and Cross-Price Sensitivity

	Bundle utility	Cross-price sensitivity
Dry grocery	0.41	-2.61
Frozen food	0.26	-1.13
Dairy	-0.25	1.82
Deli	-0.74	0.56
Packaged meat	-1.30	0.40
Alcoholic beverages	-0.18	0.90

2.5.3. The Change in Price Sensitivity

We now calculate the changes in price sensitivity based on the model estimates. The stylized model presented in Section 2 suggests that due to the (decreased) transportation cost, customers may become more price sensitive after assortment expansion, and this effect is heterogeneous across customers and categories depending on their characteristics. Figure 5 summarizes the predictions from the model in our empirical context. In particular, customers are more price sensitive for categories that they don't have a strong preference for and are complements (or weak substitutes) to liquor. After liquor was introduced, demand for these categories moves to a steeper part of the curve. In this section, we examine whether this phenomenon arises in our setting.

To determine the change in price sensitivity due to assortment expansion, we need to make sensitivity calculated under different market conditions comparable. Instead of comparing

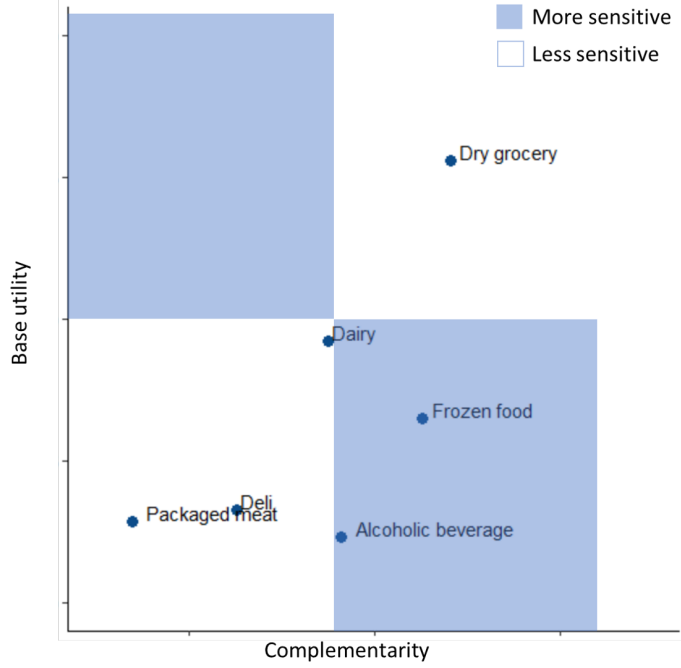


Figure 5: Hypothesized Changes in Price Sensitivity across Categories

the observed price sensitivity before and after assortment expansion, we compare the price sensitivity implied by two scenarios, with and without transportation costs while holding the other demand parameters, e.g., fixed effects (η_{ct}), the same as the after period. In this way, we can distinguish the changes in price sensitivity due to assortment expansion from those due to, for example, seasonal changes in demand.

In particular, customer i 's price sensitivity for category c in trip t given demand parameters Θ_i is

$$\begin{aligned}
 e_{ict}(\alpha_i, \beta_i, \gamma_i, s_i, X_{it} | \Theta_i) &= \frac{\partial Q_{ict}(\Theta_i)}{\partial p_{ict}} \\
 &= \frac{\partial}{\partial p_{ict}} \left(\frac{\sum_{b:c \in b} \exp(v_{ibt})}{1 + \sum_b \exp(v_{ibt})} \right) \\
 &= \beta_i \cdot \frac{\sum_{b:c \in b} \exp(v_{ibt}) \left(1 + \sum_{b:c \notin b} \exp(v_{ibt}) \right)}{\left(1 + \sum_b \exp(v_{ibt}) \right)^2} \\
 &= \beta_i Q_{ict}(\Theta_i) [1 - Q_{ict}(\Theta_i)].
 \end{aligned} \tag{2.16}$$

Averaged across trips, the customer's price sensitivity for category c under demand parameters Θ_i is given by

$$e_{ic}(\Theta_i) = \frac{1}{|T_i^{after}|} \sum_{t \in T_i^{after}} e_{ict}(\Theta_i). \quad (2.17)$$

And the trip-frequency-weighted average price sensitivity across customers is given by

$$e_c(\Theta_i) = \frac{1}{\sum_i |T_i^{after}|} \sum_i \sum_{t \in T_i^{after}} e_{ict}(\Theta_i). \quad (2.18)$$

Panel A of Table 10 reports the average change in price sensitivity in each of the six categories. Contrary to the conventional belief that more is always better, price sensitivity increases after assortment expansion in two out of the six existing categories. Specifically, in the frozen food and alcoholic beverages categories, which are complement and weak substitute for liquor, customers become more price sensitive, confirming our hypotheses. Panel B of Table 10 summarizes the distribution of the change in price sensitivity at the individual level. There is substantial heterogeneity across individuals in all six categories. In Figure 9, we plot customers' changes in price sensitivity for six existing categories against their baseline preference for the category and the preference for co-purchase (of liquor and that category). There is a clear pattern. Consistent with our hypotheses, a customer becomes more price sensitive when she has weak baseline preference for the category, and the category is a complement for liquor. In the frozen food category, more than 80% of customers experience an increase in price sensitivity, and in the alcoholic beverage category, 97% of customers become more price sensitive post assortment expansion. Customers become less price sensitive in categories for which she has a strong preference and are complements for liquor. In this context, there is no such category where customers have a strong preference and enjoy co-purchase with liquor. Overall, averaged across the six categories, price sensitivity increases post assortment expansion in 42% of the cases.

Next, we examine whether the change in price sensitivity is related to a customer's observed characteristics, including their family income and the liquor store density in the residential

Table 10: Changes in Price Sensitivity and Its Heterogeneity

	Dry grocery	Frozen food	Dairy	Deli	Packaged meat	Alcoholic beverage
<i>Panel A. Average</i>						
Mean change ($\times 10^{-3}$)	1.0	-2.1	0.1	0.2	1.2	-1.1
95% CI ($\times 10^{-3}$)	[0.0, 1.3]	[-8.4, 3.0]	[-0.4, 0.4]	[-0.7, 0.7]	[0.1, 1.5]	[-1.4, 0.0]
Mean percentage change	1.2%	-1.8%	0.0%	0.3%	1.9%	-2.6%
95% CI	[0.0%, 1.6%]	[-7.3%, 2.6%]	[-0.2%, 0.2%]	[-0.9%, 0.9%]	[0.2%, 2.4%]	[-3.3%, 0.0%]
<i>Panel B. Distribution across customers</i>						
% negative (more sensitive)	2%	83%	60%	22%	2%	97%
% positive	98%	17%	40%	78%	98%	3%
<i>Panel C. Observed heterogeneity</i>						
Density in 1st quantile	1.0%	-1.1%	0.2%	0.4%	1.9%	-2.4%
2nd quantile	1.2%	-1.7%	0.2%	0.3%	1.9%	-2.6%
3rd quantile	1.3%	-1.8%	0.1%	0.3%	2.1%	-2.9%
4th quantile	1.2%	-2.7%	0.0%	0.2%	1.8%	-2.4%
Income in 1st quantile	2.4%	-2.1%	0.1%	0.7%	2.9%	-1.8%
2nd quantile	0.8%	-1.9%	0.1%	0.2%	1.9%	-2.5%
3rd quantile	1.0%	-1.6%	0.2%	0.1%	1.5%	-2.6%
4th quantile	0.5%	-1.7%	0.2%	0.0%	1.3%	-3.5%

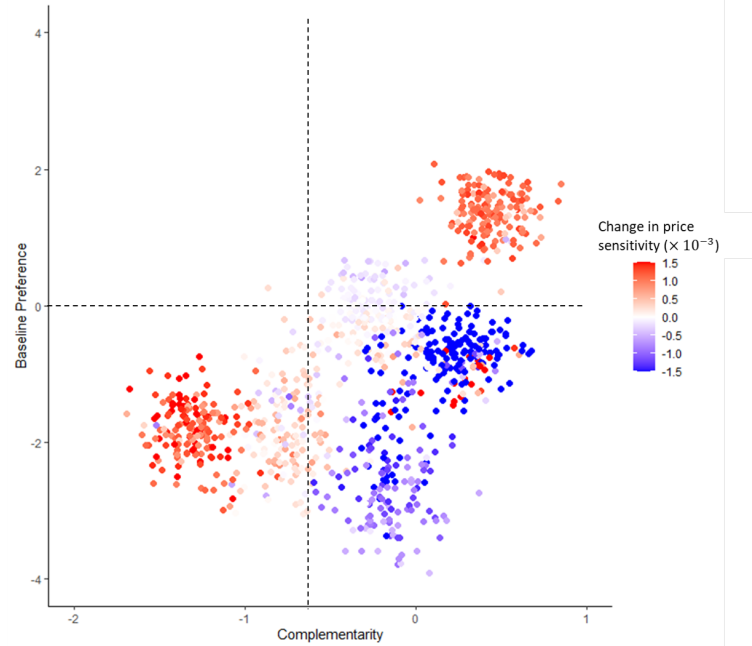


Figure 6: Changes in Price Sensitivity at the Individual Level Across Categories

area. We note that the direction and magnitude of the change in price sensitivity are jointly determined by baseline preference, preference for co-purchase with liquor, and transportation cost, which may have complex interactions depending on observed characteristics. It is then an empirical question which group of customers has the largest change in price sensitivity. Panel C in Table 10 presents the change in price sensitivity for customers with different observed characteristics. Interestingly, the size of the effect is not always the largest for customers in areas with fewer liquor stores. In the frozen food category, customers who live in the area with the highest density of liquor stores increase their price sensitivity by 2.7% while customers in low density areas only increase their price sensitivity by 1.1%. This is because customers with higher transportation cost have the weakest preference for frozen food. In the dry grocery, packaged meat, and alcoholic beverages category, however, customers who live in the area with modestly high density of liquor stores experience the largest change in price sensitivity. On the other hand, the high income group has the largest increase in price sensitivity for dry grocery and the smallest increase in price sensitivity for alcoholic beverages.

2.5.4. Profit Loss

Our previous results suggest that customers are more price sensitive while purchasing some of the existing categories when a new category liquor is introduced. In this section, we expand our results by commenting on its economic significance using a series of simulations. We calculate the potential profit loss if the retailers do not take into account this effect. Similar to Section 2.5.3, we compare the implied profits with and without transportation costs holding the other demand parameters the same as the after period.

We now describe the framework in which we calculate the optimal prices and profits in different demand conditions. We note that ignoring the change in price sensitivity for a product category may affect prices and profits in this category for several reasons. First, retailers will change their pricing behavior in the focal category, assuming that prices in other categories remains the same. Second, due to the cross-price effects, the pricing decisions in different categories can be strategic complements or substitutes. Hence, when the change in price sensitivity for one category is ignored, prices in other categories may deviate from their optimal value, affecting prices in the focal category. Our analysis focuses on evaluating the economic significance of the change in price sensitivity (the former effect) and abstract away from potential equilibrium effects (the latter effect). Hence, we calculate the optimal prices and profits on a category-by-category basis for each store, holding still the prices in other categories at the observed levels. Formally, the weekly store-category prices p_{sct} maximize

$$\begin{aligned}\Pi_{sct}(p_{sct}|\Theta_i, p_{sc't}) &= (p_{sct} - mc_{sct}) \hat{s}_{sct}(p_{sct}|\Theta_i, p_{sc't}) \\ &= (p_{sct} - mc_{sct}) \sum_{i \in I_s} \hat{D}_{ict}(p_{sct}|\Theta_i, p_{sc't}).\end{aligned}\tag{2.19}$$

where I_s denotes the set of customers of store s in the estimation sample. $\hat{D}_{ict}(p_{sct}|\Theta_i, p_{sc't})$ is the model's predictions of demand of customer i given the retailer's expectation of the demand parameters Θ_i and price $p_{sc't}$. mc_{sct} is the marginal cost of category c . We

construct the marginal costs using Nielsen PromoData, which collects information of UPC-level wholesale prices for each date from one confidential grocery wholesaler. We then aggregate the wholesale price to the category-week level. The first-order condition gives

$$D_{ict}(p_{sct}|\Theta_i, p_{sc't}) + (p_{sct} - mc_{sct}) \sum_{i \in I_s} \frac{\partial D_{ict}(p_{sct}|\Theta_i, p_{sc't})}{\partial p_{sct}} = 0. \quad (2.20)$$

Solving Equation (2.20) in the six existing categories, we derive the prices implied by each demand conditions. We then calculate the realized profits by plugging the prices into the profit function (2.19) under the true demand parameters.

As a normative benchmark, we first derive the optimal prices and the associated profits. In this condition, the retailer sets prices given the true demand parameters other than the realized time-varying demand shock. Following Hitsch (2006), we assume that the retailers do not know the time-varying demand shock and insert $\xi_{ict} = 0$ instead. By construction, retailers set optimal prices and receive the maximum profit in this condition. In the second condition, however, the retailer expect no change in demand (other than seasonality), especially the co-purchase of liquor and other categories when setting the prices.

Table 11 reports the profit loss due to ignoring the change in price sensitivity across six existing categories. The results show that retailers would experience notable profit loss across all six categories when the change in price sensitivity is ignored. Among the six categories, stores would experience the largest profit (2.2%) loss in the dry grocery category, and 95% of the stores in the data have a profit loss larger than 1.2%.

2.6. Conclusion and Discussion

In this research, we empirically examine whether assortment expansion leads to a change in the price sensitivity of demand for existing categories. We leverage a natural experiment induced by the state of Washington's deregulation of liquor markets to identify the causal effect. Looking at a representative panel's purchase behavior in the food categories, we

Table 11: Percentage Profit Loss When the Changes in Price Sensitivity are Ignored

Category	Profit Loss	5%	95%
Dry grocery	2.2%	1.2%	3.2%
Frozen foods	1.2%	0.3%	2.1%
Dairy	0.4%	0.1%	0.7%
Deli	0.1%	0.0%	0.2%
Packaged meat	1.0%	0.0%	1.8%
Alcoholic beverages	1.0%	0.7%	1.4%

find that, surprisingly, customers are on average more price-sensitive when purchasing from existing categories in the presence of liquor. Averaged across the six categories, customers become more price-sensitive post assortment expansion in 42% of the cases. Further, there is substantial heterogeneity in the changes in price sensitivity across different categories. We find that the direction and magnitude of these changes are dependent on the preference for the focal category and whether the focal category is a complement or a substitute for liquor. Consistent with the hypotheses, a customer becomes more price-sensitive when she has a weak baseline preference for the category and the category is a complement to liquor. The price sensitivity of frozen food and alcoholic beverages increase by 1.8% and 2.6%, respectively. Counterfactual analyses show that ignoring the change in price sensitivity leads to a revenue loss as large as 2.2%.

Our findings have important implications for pricing and assortment planning. For example, we demonstrate that retailers who fail to account for the fact that price sensitivity is endogenous to the set of products available and do not re-optimize price level after assortment expansion might have a loss in expected profits. The findings from our research can also help guide retailers in how they should take these changes into account and in which existing categories they should expect to see consumer demand shifts when they introduce new categories into a store.

Our study is not without limitations. First, our demand model focuses on primary demand (purchase incidence). Future work may investigate the change in the price sensitivity in a

unified framework that models both purchase incidence and quantity. Second, we investigate the effect of assortment expansion in a unique natural experiment setting. Future research could show the robustness of these effects in other settings.

In summary, our research serves as a useful first step in understanding how assortment expansion affects the price sensitivity of existing products. Quantifying these effects provides important implications for retailers' pricing and assortment planning decisions.

CHAPTER 3 : Does Privatization Lead to Lower Liquor Prices: Evidence from the State of Washington

3.1. Introduction

For a long time, many states in the U.S. controlled where and when liquor could be sold and held a monopoly of liquor sales. Starting from the 1960s, a number of states have taken action to deregulate the liquor markets and loosened their monopoly. For instance, West Virginia sold all of its state liquor stores to private owners; Vermont now permits private retailers to sell alcohol on behalf of the state for a commission. As of 2020, about two-thirds of the states have adopted private license systems. The privatizations stir widespread interests and concerns as they drastically change the market structure and affect liquor consumption. However, very little empirical work quantifies the effect of privatization on prices and other market outcomes.

We study the privatization of liquor in the state of Washington. The state of Washington was among the “control states” where the government held a monopoly on both the retailing and distribution of liquor. In 2011, Initiative 1183 privatized the sales of liquor. A license system was introduced and retailers with premises larger than 10,000 square feet were qualified to obtain a license to sell liquor starting from June 2012.

Voters supporting the privatization expected greater access, coupled with decreases in prices (e.g., Kerr et al., 2015; Subbaraman and Kerr, 2016). Due to the policy change, the number of liquor stores increased by more than 300% from 330 to around 1400 immediately after June 2012. More importantly, the new liquor-carrying stores were grocery stores where customers had their day-to-day shopping. Customers can now purchase liquor, beer, and wine together with groceries in one place. Voters also expected the liquor prices to drop because the monopoly system that “is inefficient, interferes with the free market, raises prices, limits selection” would be abandoned (Subbaraman et al., 2020).

However, another consequence of the privatization is increased fees and taxes. While Ini-

tiative 1183 stated that it “does not increase any tax, create any new tax, or eliminate any tax,” similar to many other states, it introduced new liquor retail and distribution license fees to maintain state revenue. Retail license holders were required to pay a license issuance fee equivalent to 17% of the sales revenue, and distributor license holders were required to pay 10% of their sales revenue in the first two years. Although these fees were not collected directly from the consumers, they might be passed on from the retailer to the consumers and reflected in the final prices. Combining these elements, the liquor tax rate in the state of Washington is now the highest in the US.

In addition to introducing competition and new fees, the privatization of liquor could affect its prices for reasons not explicitly stated in the initiative. As discussed in Chapter 2, the convenience of one-stop shopping leads to changes in the level as well as the price sensitivity of demand, which will, in turn, affect liquor prices. On a related point, liquor departments in grocery stores may have different pricing incentives from stand-alone liquor stores. Stand-alone liquor stores maximize the profits from liquor (and other alcoholic beverages) only. When retailers set prices across several categories, the cross-price effects are internalized. When the categories are complements, multi-category retailers set lower prices than when they are operated independently. In regions where alcoholic beverages are sold with groceries, they are sometimes priced even below the costs as loss leaders to drive customer traffic (e.g., Bennetts, 2008). The liquor prices may also change simply because private stores and state-owned stores have different types or levels of selection. Anecdotal evidence suggests that the stand-alone state-owned stores usually offer more variety of hard liquor. For instance, a state-run stand-alone liquor store in Idaho could have 76 kinds of bourbon on average, while the nearby Walmart and Target had only 12 different varieties.¹

In this chapter, we empirically examine the impact of privatization on liquor prices. In addition, as the effect of privatization may be multiple-determined, we seek to understand the mechanisms that drive the changes in prices. We start by quantifying the changes in

¹“Private liquor in Washington state: Are we better off?” <https://www.krem.com/article/news/investigations/private-liquor-in-washington-state-are-we-better-off> (accessed April 5, 2021).

liquor prices after the privatization. Our analysis is based on a difference-in-differences model that compares changes in prices before and after the privatization in Washington relative to the nearby state of Oregon. Surprisingly, we find that liquor prices increased significantly, even though there is a considerable increase in the number of liquor stores. If the privatization intensifies competition and lowers liquor prices, the observed price increase must be explained by the other mechanisms.

We then explore the effect of each of the remaining four mechanisms through a series of descriptive analyses. First, we show that a part of the increase in prices can be explained by the increased fees passed through to customers from the retailers. However, the net-of-fees prices also increase significantly after the privatization. Next, we investigate the price effect of one-stop shopping. We find that customers are more price sensitive after the privatization. However, this would predict a lower rather than higher price. We also show that the private stores and state-owned liquor stores have different selections of liquor. Yet, customers paid more even for the same product. Finally, we find that there is a cross-price effect between liquor and existing grocery categories. A decrease in liquor price is associated with an increase in the demand for grocery items. This indicates that the retailers have the incentives to set lower liquor prices to boost the sales of grocery items and maximize the joint profits. Taken together, the observed price increase is not explained by the latter four mechanisms. This leaves us to conclude that increasing store density does not lead to lower prices in the state of Washington.

After providing qualitative evidence that the changes in prices are multiple-determined and that competition may not lead to lower prices, we develop a framework to quantify the price effect of each mechanism. We build and estimate a model in which liquor prices are jointly determined by customer demand and firm price-setting behavior. On the demand side, we model customer's choices for liquor and grocery categories in a bundle utility framework. We account for customer's preference for co-purchase, one-stop shopping, different levels of liquor selections, as well as the rich heterogeneity in preference by family income and liquor

store density. On the supply side, we model multi-category retailers' pricing behavior given different fee and category ownership structures. Based on the model, we conduct a series of counterfactual simulations to calculate the price effect of each underlying mechanism.

Our results suggest that customers value the convenience of one-stop shopping, leading to a \$0.25 increase in liquor price. We also confirm the descriptive evidence that liquor is on average a complement to groceries. To take advantage of this negative cross-price effect, retailers would set the price by \$1.34 lower. On the other hand, the surge in liquor prices in the state of Washington is mainly due to the high license fees. Customers pay \$2.04 more given the new fee structure. Contrary to the voters' expectations, introducing liquor to grocery stores does not intensify competition. Rather, it results in a \$3.1 increase in price. We compare our results to the extant literature and posit some explanations.

Our findings provide guidance on the regulation of liquor. First, our results indicate that the pass-through rate in the liquor market is quite high. It is usually the consumers that bear the most tax burdens. Second, we show that although the initiative increases the number of liquor stores by threefold, it does not encourage competition. Instead, since now the convenience stores and smaller stores are prevented from entering the market, the larger grocery stores may gain even more market power and set higher prices.

The rest of the chapter is organized as follows. We review the related literature in Section 3.2. In Section 3.3, we describe the institutional details. We then describe our data and provide descriptive evidence to test the mechanisms through which the privatization of liquor could affect its prices. Section 3.4 presents a framework in which we decompose the changes in liquor prices onto different mechanisms. Section 3.5 presents the results from the decomposition. Section 3.6 concludes.

3.2. Literature Review

Our research connects literature on competition, multi-category retailing, and taxation to address the policy debate on the privatization of liquor markets. In what follows, we review

each stream of literature and the studies on liquor markets in general.

3.2.1. Competition and Market Outcome

Our research is closely related to the literature on competition and market outcomes. Prior literature has empirically explored how the number and organization of firms in a market, firms' sizes, potential competitors, and product lines affect competition and firm profits (e.g., Bresnahan and Reiss, 1991, see also Berry and Reiss, 2007 for a review). While classic economic theory predicts that competition leads to lower equilibrium prices, mixed evidence is found. The closest to our research is Illanes and Moshary (2020), which use a reduced-form approach to study the relationship between market structure and market outcomes in the same empirical context. They find no effect of increased competition on prices as retailers adopt a uniform pricing strategy and do not set prices based on the local competition. We show that by allowing grocery stores to carry liquor, average prices increase. Instead of encouraging competition, the larger grocery stores may gain even more market power as their competitors of smaller sizes are disqualified from entering the market.

3.2.2. Multi-category retailers

Our research also adds to the small but growing literature on multi-category retailers. Lal and Rao (1997), Bell et al. (1998), and Bell and Lattin (1998) study the practice of Every-Day-Low-Pricing and High-Low-Pricing strategy of the supermarkets. Simester (1995), Rosato (2016), and Chen and Rey (2012) investigate the loss leader strategy commonly adopted by multi-category retailers. They find that large retailers can price below cost in some categories in order to signal the prices of other categories in the store, bait customers, and discriminate multi-stop shoppers from one-stop shoppers. Thomassen et al. (2017) estimate a multi-product multi-store demand model and find complementary pricing effects between categories sold by the same retailer. Supermarkets should set prices lower than the independent category sellers. Rhodes (2015) theoretically examines the relationship between firm scope and prices. He also shows that larger stores should charge lower prices because they attract consumers with lower product valuations. Sweeting (2010) studies the

pricing and product positioning of multi-product firms and finds that larger firms choose to position their products closer to competitors to soften price competition. We study how multi-category retailers can utilize the cross-price effects to maximize joint profits. In particular, we find a strong cross-price effect between liquor and grocery items and quantify its impact on prices.

3.2.3. Taxation

This work also contributes to the literature on taxation. Recent research on liquor taxes has found large pass-through rates. For example, Ardalan and Kessing (2019) document a pass-through rate of 93%. Generally, the pass-through rates range from 50% to more than 100% (see Nelson and Moran, 2019 for an excellent review on this topic). We find that the demand for liquor is less elastic than the supply for liquor. Hence, a majority of the license fees (53%) are passed through to customers.

3.2.4. Deregulation and Privatization of Liquor Markets

This research adds to the growing literature on the deregulation of the liquor market. For instance, Milyo and Waldfogel (1999) study Rhodes Island’s elimination of the ban on liquor advertising. Huang et al. (2020) examine private retailers’ learning behavior when they are inexperienced after the privatization of liquor and have to learn about the demand for liquor in Washington. Seo (2019) evaluates the gains in consumer welfare due to the privatization of liquor in Washington. Our research complements these studies by quantifying the price effects of various components commonly seen in privatization and provide guidelines for policymakers.

3.3. Institutional Background and Data

3.3.1. Privatization of Liquor in the State of Washington

The state of Washington was among the most heavily regulated states when it comes to the sales of liquor. It held a monopoly on both the retailing and distribution of liquor from 1933 to 2012. By early 2012, there were only 330 liquor stores in Washington; 167

of them were directly operated by the state and the rest of them were operated by state contractors. In November 2011, Initiative 1183 was approved to privatize liquor sales in the state of Washington from June 2012 onward. This initiative ended the state monopoly in the liquor market. The state-owned stores were auctioned off to private retailers. Private retailers with premises larger than 10,000 square feet were allowed to apply for a license to sell liquor. Priority was given to those who already held a retail license for beer and wine. Among the new liquor stores were the existing beer and wine superstores and the grocery stores such as Costco and Walmart. By the end of 2012, the number of liquor stores increased by more than 300%, from 330 to around 1400.

In addition to increasing the availability of liquor, the initiative also included new taxes and fees to maintain the revenue. The state kept a 20.5% liquor sales tax plus a volume tax of \$2.83 per 750 ml bottle. These taxes were collected directly from the consumers. The state also imposed a retail license fee that equals 17% of the total revenue and a wholesale license fee that is 10% of the total revenue, making the liquor tax rates in the state of Washington the highest in the US. The initiative predicted an increase in state revenues of \$216 to \$253 million and an increase in local revenues of \$186 to \$227 million over six years.

3.3.2. How and Why Do Liquor Prices Change

A primary argument of the initiative's proponents is that privatization would lead to lower prices because it encourages competition (Subbaraman and Kerr, 2016). While this conjecture is consistent with basic economic theory, past research provides mixed evidence of whether competition has a meaningful impact on prices. In addition, since the changes occurring in Washington go beyond simply increasing the store density, it is unclear *ex-ante* how the price will change and what the main drivers of the change are. In fact, liquor prices increase after the privatization. Figure 7 shows the liquor prices in the state of Washington in 2012. There is a spike when the initiative came into effect in June. Clearly, the liquor prices in the state of Washington are influenced by the interaction of multiple mechanisms.

In what follows, we first quantify the changes in liquor prices after the privatization. We

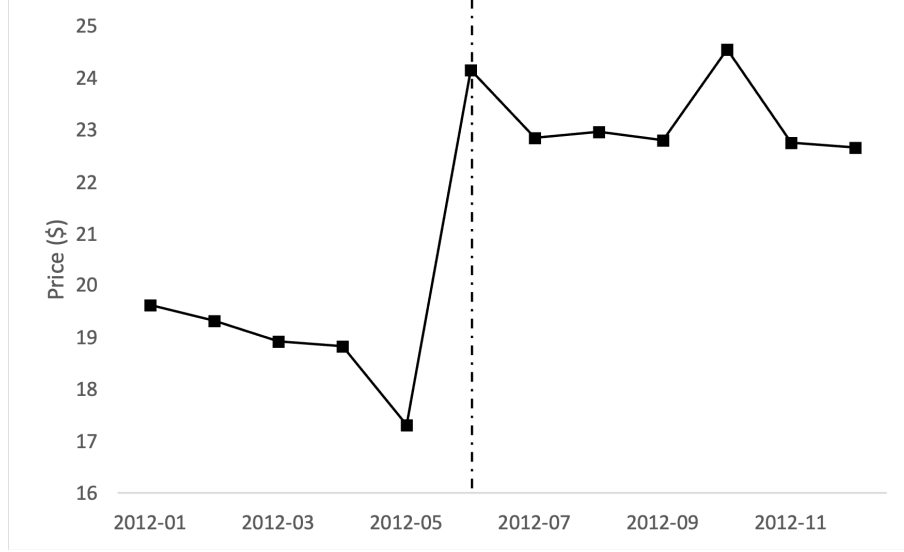


Figure 7: Washington Liquor Prices in 2012

then discuss the price effect of each underlying mechanism. We use a difference-in-differences model that compares changes in prices in Washington with the control state Oregon.² We estimate the following specification:

$$y_{pt} = \beta_0 + \beta_1 \text{Treated}_p + \beta_2 \text{After}_t + \beta_3 \text{Treated}_p \cdot \text{After}_t + \epsilon_{pt}. \quad (3.1)$$

Here, y_{pt} is the outcome of interest, the average liquor price for state p in week t , Treated_p is an indicator variable for the state of Washington, and After_t is an indicator variable that takes 1 after the presence of liquor and zero otherwise. Column (1) in Table 12 reports the results from the model. We confirm the patterns in Figure 7; liquor prices increase by \$4.67 after the privatization. If the privatization intensifies competition and lowers liquor prices, this price increase must be explained by the other mechanisms.

Recall that the state imposes a 17% retail license fee and a 10% wholesale license fee. How much these fees are passed through to the consumers depends on the elasticity of demand relative to supply, with the less elastic side bearing the most burden. Recent studies on alcohol taxation find pass-through rates close to or higher than 100% (e.g., Ardalan and

²See Chapter 2 for a discussion on the choice of the control group and a placebo test.

Kessing, 2019, also see Nelson and Moran, 2019 for a review), suggesting that it is usually the consumers that bear most of the tax impact. To test how much of the price increase is driven by the fees, we estimate the change in the net-of-fees prices (i.e., retailers' revenue per unit net of license fees) before and after privatization using Equation (3.1). Column (2) in Table 12 reports the results from the model. We find that net-of-fees prices also increase by \$2.58. The changes in liquor prices are not solely driven by the license fees.

The convenience of one-stop shopping leads to changes in the level of demand as well as the price sensitivity of demand, which will in turn affect liquor prices. We identify the changes in the level and sensitivity of demand using a difference-in-difference model, again with Oregon customers being the controls. Specifically, we estimate the following regression model:

$$y_{it} = \beta_1 \text{Treated}_i \cdot \text{After}_t + \beta_2 \text{Price}_{it} + \beta_3 \text{Price}_{it} \cdot \text{Treated}_i + \beta_4 \text{Price}_{it} \cdot \text{After}_t + \beta_5 \text{Price}_{it} \cdot \text{Treated}_i \cdot \text{After}_t + \alpha_i + \gamma_t + \epsilon_{it}, \quad (3.2)$$

where y_{it} and Price_{it} are the purchase incidence and price of liquor, respectively. Treated_i is an indicator variable that equals one if i is a Washington customer and zero otherwise, After_t is an indicator variable that takes 1 after grocery stores started to carry liquor and zero otherwise, α_i are individual fixed effects, and γ_t are week fixed effects. The parameters of interest are β_1 and β_5 , which measure the change in the level and the price sensitivity of demand. Column (3) in Table 12 reports the results from the regression. Not surprisingly, the demand for liquor increases after the privatization ($\beta_1 = 0.19$, $p < 0.01$). We also find that the price sensitivity for liquor increases ($\beta_5 = -0.07$, $p < 0.01$). This is consistent with the predictions in Chapter 2. The reduction of transportation cost increases the utility a customer gets from the purchase of liquor. Since customers on average have low demand for liquor and that other groceries complement liquor, the demand for liquor moves towards the right and closer to the “tipping point” of the S-shaped demand curve and price sensitivity increases. Therefore, an optimal retailer should lower liquor prices, which is inconsistent with the observation that liquor prices increase after the privatization. We conclude that

the increase in price sensitivity cannot fully explain the change in liquor prices.

Table 12: Changes in Net-of-fees Price, Demand, and Price Index

	Dependent variable			
	Price (1)	Net-of-fees Price (2)	Purchase Incidence (3)	Price index (4)
Treated · After	4.67*** (0.19)	2.58*** (0.23)	0.19*** (0.03)	4.78*** (0.14)
Treated · After · Price			-0.07*** (0.03)	
Observations	24	24	110,424	24
Adjusted R-squared:	0.181	0.125	0.119	0.267

Note: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$
Robust standard errors in parentheses.

We next explore the price changes due to the cross-price effects. When grocery stores start to carry liquor, the cross-price effects between liquor and groceries are internalized, which may affect liquor prices as well. As discussed in Chapter 2, we find liquor on average is a complement to existing grocery categories. This, however, suggests that multi-category retailers should set lower prices than they would when the categories are operated independently to take advantage of the negative cross-price effects.

Lastly, we investigate whether the changes in prices can be explained by the different selections of liquor in private and state-owned stores. The observed change in prices may confound two effects: differences in prices of the same products and differences in selection and/or liquor quality. To separate the latter from the former, we examine the changes in prices of the same set of products by estimating Equation (3.1). The outcome variable y_{st} is a price index calculated as the weighted average price of 718 UPCs sold throughout 2012 and in both treatment and control states. The weights are chosen to be the total sales of each UPC in 2012. We find customers pay \$4.78 more even for the same set of products. This also implies that the average prices would be even higher if the grocery stores were to offer the same assortment as the stand-alone liquor stores.

In summary, we provide suggestive evidence that multiple forces affecting liquor prices are in play. We find the license fees only explain a part of the increase in prices. Increased price sensitivity, cross-price effects, and the change in selection all predict a price change opposite to the observed one in the direction. Overall, these results imply that increasing store density leads to higher prices on net.

3.4. Empirical Framework

In this section, we develop a model of consumer and firm behavior. Explicitly modeling firm pricing behavior allows us to systematically quantify the price effects of each mechanism. In what follows, we first present our empirical framework. Then, we describe how we capture the effect of each mechanism in this framework.

3.4.1. Demand

Our demand model follows the Bayesian Hierarchical Choice model developed in Chapter 2. The model characterizes a customer's multi-category purchase with transportation costs. The model also accounts for seasonal changes in category preference, price endogeneity, and heterogeneity by family income and liquor store density. We refer to Chapter 2 for a detailed description and estimation results of the demand model. Briefly, in each shopping trip, a customer chooses the bundle (combination of categories) that gives her the highest utility. Customer i 's utility of purchasing bundle b in shopping trip t is

$$u_{ibt} = \sum_{c \in b} \left[\alpha_{ic} + \sum_{m=2}^{12} \eta_{cm} \mathbf{I}(t \in m) + \xi_{ict} + \beta_i p_{ict} \right] + \sum_{c \in b} \sum_{c': c' \in b, c' < c} \Gamma_{i\{c, c'\}} - s_{ibt} + \epsilon_{ibt} \quad (3.3)$$

where α_{ic} is the baseline preference for each category, η_{cm} are category-month fixed effects, β_i is the price coefficient, p_{ict} is the price of the product category c , ξ_{ict} is unobserved demand shocks addressed by the control function approach (Petrin and Train, 2010), Γ_{ib} is the bundle utility term, and s_{ibt} is the transportation cost occurred in trip t when buying bundle b . The customer pays the transportation cost if she buys both liquor and groceries before the privatization of liquor and thus visits more than one store within a trip, i.e.,

$s_{ibt} = s_i \cdot I(t \in T^{before}) \cdot I(\text{liquor} \in b)$ where s_i is the transportation cost for two-stop shopping. Finally, ϵ_{ibt} is the error term that is assumed to follow i.i.d. type-I extreme value distributions.

Several findings emerge from the model. First, customers value the convenience of one-stop shopping. A customer is willing to pay \$5.6 to purchase groceries and liquor within one stop. Second, liquor purchases are dependent on purchases in other categories. Table 13 presents the average own- and cross-price elasticity based on the estimates of the demand model. The (i, j) element in the table is the elasticity of category i with respect to a change in the price of category j , evaluated at the observed price levels. The own-price elasticity of liquor is -2.73, which is comparable with the extant literature. For instance, Baltagi and Griffin (1995) find that the own-price elasticity of liquor is -2.03. We also note that the cross-price effects are not negligible. For example, the demand for liquor decreases when there is an increase in the price of dry grocery and frozen food because they are complements. At the same time, the cross-price effects between liquor and other alcoholic beverages, e.g., beer and wine, are large and positive because they are substitutes. Third, the baseline demand for liquor changes after its privatization, even in the absence of co-purchase. This may be due to the seasonal demand for liquor. It is also possible that the selections of liquor in the grocery stores differ significantly from those in the state-owned liquor stores. Put together, these results suggest that the privatization of liquor affects the demand for liquor in multiple ways. Finally, we note that there is substantial heterogeneity across customers in their demand parameters. Consistent with expectation, the value of one-stop shopping is especially large for customers who reside in areas with a lower density of state-owned liquor stores prior to the privatization of liquor. Therefore, the effect of privatization on liquor prices may vary across stores in different areas.

3.4.2. Supply

We use counterfactual analysis to study the mechanisms through which the privatization of liquor affects liquor prices. Motivated by the discussion in the last section, we consider

Table 13: Own- and Cross-Price Elasticity

	Liquor	Dry Grocery	Frozen Food	Dairy	Deli	Packaged Meat	Alcoholic Beverages
Liquor	-2.73	-0.06	-0.13	0.01	0.09	0.18	0.14
Dry grocery	-0.23	-0.30	-0.17	-0.16	-0.13	-0.24	-0.04
Frozen food	-0.22	-0.08	-1.04	-0.16	-0.12	-0.21	-0.03
Dairy	0.00	-0.05	-0.11	-0.42	-0.05	-0.22	0.01
Deli	0.09	-0.04	-0.07	-0.04	-1.70	0.11	0.11
Packaged meat	0.15	-0.05	-0.11	-0.15	0.09	-1.82	0.01
Alcoholic beverages	0.11	-0.01	-0.01	0.01	0.08	0.01	-1.92

five mechanisms (i) change in demand due to one-stop shopping (ii) cross-price effects (iii) change in liquor selection and quality (iv) change in tax rate (v) competition. To calculate the price effects of each mechanism, we obtain the prices implied by the following six scenarios described in Table 14. These include four counterfactual scenarios as well as two observed scenarios (before and after the privatization of liquor). We start from the observed prices after the privatization. In this scenario, all six mechanisms are in play. Each scenario onward turns off the effect of one mechanism. The difference between the prices in two consecutive scenarios gives the effect of each mechanism. For example, the effect of the competition is identified by comparing the counterfactual prices in scenario 4, which turns off the first four mechanisms and assumes that the privatization only affects the number of liquor stores in the market, with the observed prices before the privatization when the market structure has not been altered. In what follows, we first present a general pricing model to calculate prices given arbitrary levels of demand and market structure. In this model, multi-category retailers set prices to maximize their total profits. Then, we discuss each mechanism in detail and show how we operationalize them in this model.

Pricing Model

Suppose there are G stores, each of which carries some subset, Δ_m , of the $c = 1, 2, \dots, C$ categories in the market. Throughout this section, we suppress the time subscript t for

Table 14: Counterfactual Simulation Setup

Mechanism	One-stop shopping	Cross-price effect	Change in liquor selection	Change in tax rate	Competition
observed after	Yes	Yes	Yes	Yes	Yes
1	No	Yes	Yes	Yes	Yes
2	No	No	Yes	Yes	Yes
3	No	No	No	Yes	Yes
4	No	No	No	No	Yes
observed before	No	No	No	No	No

parsimony. The profit of store g is given by

$$\Pi_g = \sum_{c \in \Delta_g} ((1 - \tau_c)p_{gc} - mc_{gc}) D_{gc}(p_{gc}|\Theta, p_{g'c'}), \quad (3.4)$$

where p_{gc} is the after-sales-tax category price faced by the customers, τ_c is the retail tax rate faced by the retailers. mc_{gc} is the marginal cost. $D_{gc}(p_{gc}|\Theta, p_{g'c'})$ is the demand for category c in store g given the prices and demand parameters Θ . We will discuss the demand parameters in greater detail in the next few paragraphs and show how we can capture the effect of different mechanisms on demand. Assuming that each store sets the category prices to maximize the total profit from its assortments, the first order conditions are

$$D_{gc} + (p_{gc} - mc_{gc}) \frac{\partial D_{gc}}{\partial p_{gc}} = 0. \quad (3.5)$$

Written in a vector form, the price vector p satisfies

$$p = \frac{1}{1 - \tau} \circ \left[mc - \left(\Omega \circ \frac{\partial D}{\partial p} \right) \cdot D \right]. \quad (3.6)$$

Here Ω is a $C \times C$ ownership matrix. The (i, j) element of the matrix Ω takes 1 if category i and j are both sold in store s and zero otherwise. The operator \circ is elementwise matrix multiplication.

The marginal cost of liquor

To obtain the marginal cost of liquor and other categories, we solve for the vector of mc that satisfies (3.6) for each store given the observed prices. Throughout this section, we take these marginal costs as given. The implicit assumption is that after privatization, retailers set prices by maximizing total profits across categories. While some other research suggests that retailers may deviate from optimal (e.g., Goldfarb and Yang, 2009; Huang et al., 2020), our results are not sensitive to the specification of marginal costs.

One-stop shopping

Counterfactual scenario 1 turns off one-stop shopping. This is achieved by setting the transportation cost $s_{ibt} = s_i \cdot I(\text{liquor} \in b)$, i.e., customers pay the transportation cost post assortment expansion.

Cross-price effects

Counterfactual scenario 2 further turns off cross-price effects, which we manipulate by change the ownership matrix Ω . Suppose the first column and row of the matrix is the ownership of liquor. The ownership matrix with joint pricing is given by Ω_1 , with all the elements being one. Without joint pricing, profits from liquor and groceries are maximized independently. The ownership matrix is a block matrix Ω_2 .

$$\Omega_1 = \begin{pmatrix} 1 & 1 & \dots & 1 \\ 1 & 1 & \dots & 1 \\ \vdots & \vdots & \ddots & \vdots \\ 1 & 1 & \dots & 1 \end{pmatrix}, \quad \Omega_2 = \begin{pmatrix} 1 & 0 & \dots & 0 \\ 0 & 1 & \dots & 1 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 1 & \dots & 1 \end{pmatrix}.$$

Change in liquor selection and quality

Counterfactual 3 requires the quality of the categories post-privatization to be the same as they were before. We set the monthly fixed effect after June 2012, $\eta_{cm} = 0$.

Change in tax rates

Counterfactual four studies the tax impact on liquor prices. To do that, we plug the respective tax rates into τ_c .

3.5. Results

Figure 8 shows how each mechanism contributes to the price changes. In Figure 9, we also present the distribution of these effects.

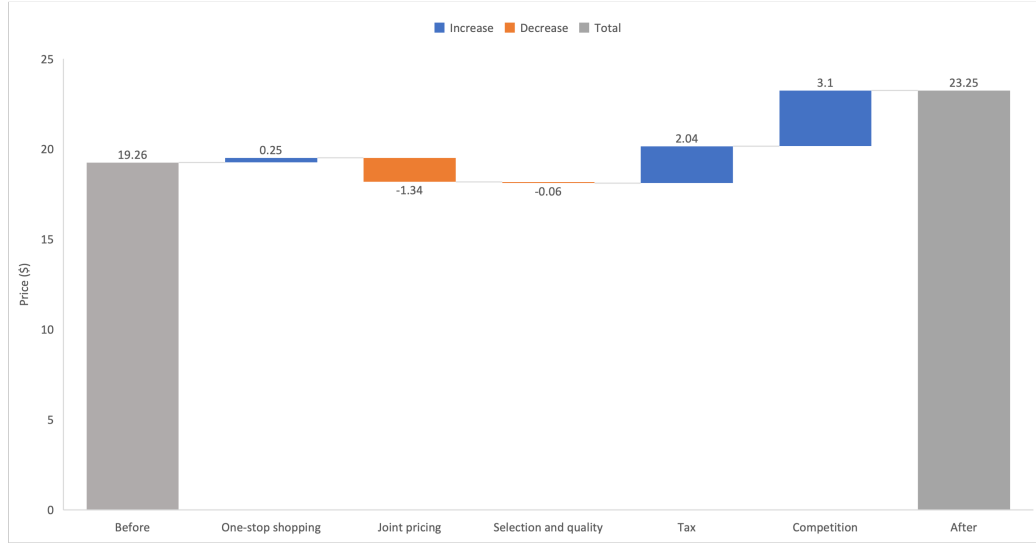


Figure 8: Decomposition of Price Changes

We first discuss how one-stop shopping contributes to the changes in liquor prices. We find that with the convenience of one-stop shopping, liquor prices would increase by \$0.25. There are two competing forces here. First, retailers may take advantage of the increase in liquor demand and set higher prices. At the same time, the demand for liquor becomes more sensitive, which forces retailers to lower their prices. In our context, the former outweighs the latter. A few stores would increase the price by more than \$1 if they co-locate with grocery stores. These are the stores located in areas with few liquor stores prior to privatization so that the savings of transportation cost are especially large.

Looking at the second mechanism, we find that liquor prices would be \$1.34 lower due to the

cross-price effects. This is consistent with the fact that liquor is on average a complement to groceries (see Chapter 2 for a detailed discussion). As a result, grocery stores have incentives to lower liquor prices, as this increases both liquor sales and grocery sales. We don't find evidence for pricing below the marginal cost. In more than 90% of stores, joint pricing lowers the prices by \$1 to \$2.

The effect of selection and quality difference on price is very small across almost all stores. Our interpretation is that while grocery stores offer much less variety than state-owned stores, customer's evaluation of the assortment does not differ significantly. On the other hand, a large portion of the price increase can be explained by the large amount of fees imposed. Specifically, the new system leads to a \$2.04 increase in price. This is consistent with the prior literature that finds large pass-through rates in the liquor markets.

Now we have discussed four out of five mechanisms associated with the policy change. These effects combined suggest that liquor prices would increase by \$0.89. We next turn to the central research question of this chapter-whether and how competition affects liquor prices. The market outcome expected by the voters was lower equilibrium prices after privatization. We find that contrary to this expectation, liquor prices are \$3.99 higher given the market structure after the privatization of liquor and \$3.10 of this increase is not explained by other mechanisms. This suggests that competition does not lower the prices in this context.

Our findings are in line with the other data patterns documented in the recent works on Washington state's liquor market. Huang et al. (2020) find that the cross-elasticities between stores are close to zero in the state of Washington. This suggests that each store operates independently in their perspective markets as monopolists even after the store density increases. In the same context, Illanes and Moshary (2020) find the liquor prices do not vary with the number of retailers in the state of Washington leveraging a regression discontinuity design. We note, however, that our findings have different implications from Illanes and Moshary (2020). The analyses in Illanes and Moshary (2020) leverage the cross-sectional variation in the number of retailers across markets after the privatization created

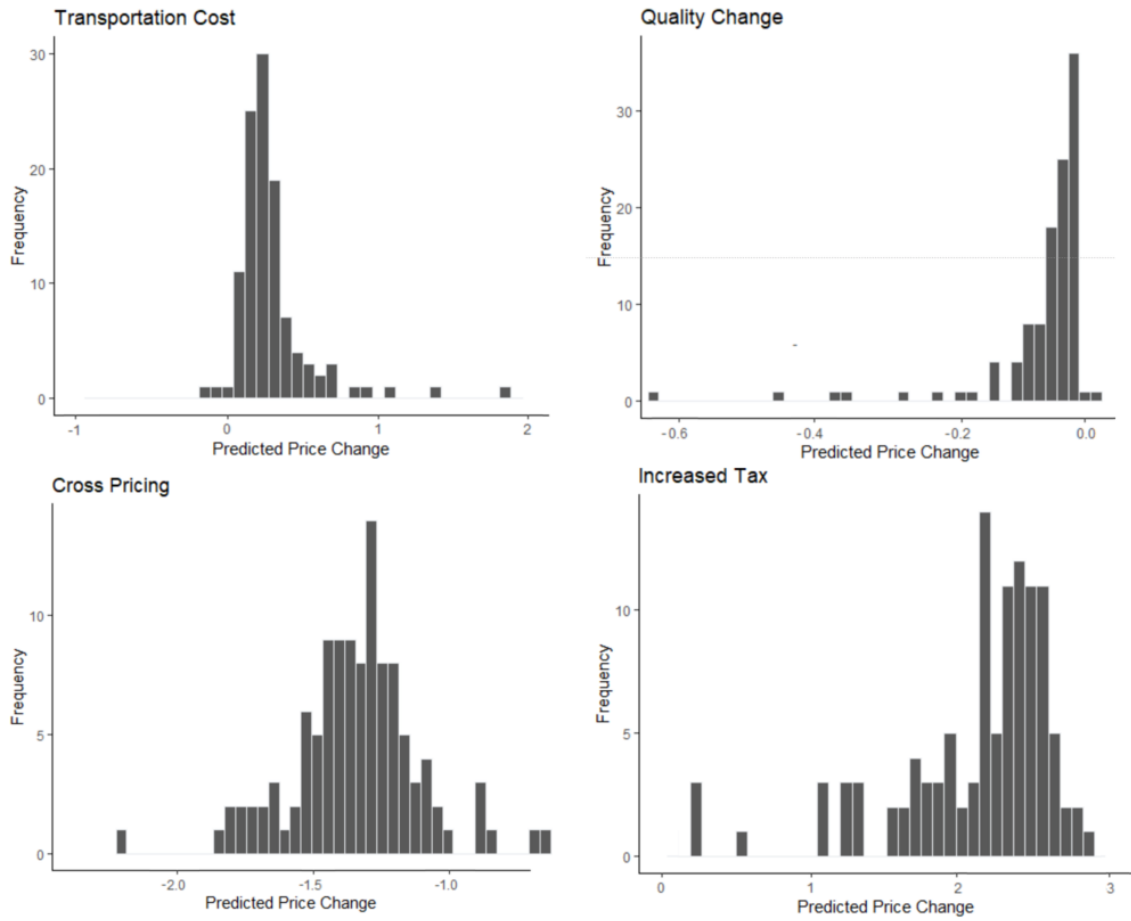


Figure 9: Distribution of the Price Effects

by the license threshold. Hence, their results speak to the intensive margin of moving from monopoly to duopoly or triopoly. Our results essentially identify the change in liquor prices when the number of stores increases by threefold due to privatization. We find that liquor prices do not decrease even though there is a larger shock to the market structure. The larger grocery stores may gain more market power and set higher prices since now the convenience stores and smaller stores are prevented from entering the market.

3.6. Conclusion and Discussion

In this research, we examine how and why privatization impacts liquor prices in the state of Washington. We propose five mechanisms through which liquor prices are affected by the policy change and develop a framework to analyze the price effect of each mechanism. We show that the initiative fails to lower liquor prices by introducing competition. There is even a surge in liquor prices in the state of Washington due to the high license fees.

Our research yields several conclusions. We show that the liquor industry has a high pass-through rate. Consumers are typically the ones that bear the brunt of tax burdens. We also find that by excluding the small stores and convenience stores, liquor prices increase after privatization, which has important implications for designing license requirements.

There are several extensions that could be made to our research. Currently, we study the demand for the liquor category as a whole. We do not distinguish different types and brands of liquor and attribute the changes in the (baseline) demand for the category to the changes in the average quality. Future work can build a UPC level model to study how retailers jointly determine the selections and prices in a store. In addition, we focus on the price changes immediately after the privatization in this research. It is also interesting to study the long-term impact of the privatization of liquor. On the demand side, as liquor is an addictive substance, customers' consumption habits may change over time. On the supply side, while there is no evidence that retailers are gaming the licensure threshold in the short term, smaller stores may strategically expand their space in the long term to be qualified for the license. It is possible that the market becomes more competitive and the prices go

down in the long term.

CHAPTER 4 : The Impact of Subscription Programs on Customer Purchases

4.1. Introduction

In an effort to retain and develop customers, retailers and marketplace platforms are increasingly turning to subscription programs, which are designed to keep customers engaged by giving access to exclusive benefits for a fee upfront.¹ For example, Amazon Prime offers members free shipping, audio, and video content, as well as member-exclusive discounts for an upfront payment of \$119 per year. Many other retailers, such as Barnes & Noble, Sephora, and Alibaba have similar programs with benefits that range from unlimited free shipping to member-only discounts and additional loyalty points.

As the relevance and popularity of subscription programs grows, it is of managerial interest to examine the causal effect of such programs on customer behavior and investigate underlying drivers for their success. For instance, an industry report speculates that Amazon Prime is quite successful, as members spend \$1,300 per year, which is almost double the average non-member's annual spending of \$700.² However, the reported difference in spending between members and non-members may arise due to several reasons. First, members likely self-select into the subscription. The naïve comparison in spending described above likely over-estimates the effect of the program, as customers who expect to make more purchases in the future are more likely to join the subscription. Second, members may change their purchase behavior due to the economic benefits they receive post subscription. Third, mere membership can also bring value and change customer behavior, for instance, by leading them to form a new consumption habit or to feel enhanced status. The industry report cited above indicates that 82% of Prime members shop with Amazon even when the price is lower elsewhere, suggesting that the impact of subscription programs can go beyond the economic

¹We distinguish subscription programs from stand-alone subscription services (e.g., Stitch Fix, Birchbox) that provide subscribers new items or personalized experiences periodically. We focus on a setting where a subscription program is initiated by an existing non-contractual business and provides members exclusive benefits beyond those available to regular customers (i.e., non-members). See Section 4.2 for a discussion on different types of subscriptions.

²"Here's How Much Amazon Prime Customers Spend Per Year," available at <http://fortune.com/2017/10/18/amazon-prime-customer-spending/> (accessed April 5, 2021).

benefits offered. From a theoretical perspective, it is important to identify the economic and non-economic effects of subscription on customer behavior separately. While the economic effect of a program may be specific to the features of the program, the non-economic effect due to the underlying psychological drivers is likely to be applicable in other contexts. Determining the relative contribution of the two components is substantively important to improving the design of subscription programs. As an extreme scenario, if additional sales are generated only by reducing (effective) prices, the program might negatively affect a firm’s performance in the long term (e.g., Raghurir 2004).³

The purpose of this paper is to take a first step towards assessing the causal impact of customers joining a subscription program on their purchase behavior. We hereafter refer to the impact as the treatment effect of subscription. We also seek to decompose the treatment effect into the economic effect due to program benefits and the non-economic effect that cannot be explained by the tangible benefits of the program (e.g., status from the program). Specifically, we are interested in addressing the following questions: Does a subscription program generate value for a firm? Is the subscription program effective in inducing customers to change their behavior because of the economic benefits and/or the psychological drivers? How do these effects vary over time and across customers? What are the underlying drivers of any documented effects? We address these questions in close collaboration with a company that launched a subscription program at its online channel. The program offers members a few exclusive benefits for an upfront fee. Our data contains individual-level transactions before and after the launch of the program and other information on various components in the program, thus allowing us to examine the effect of subscription on purchase behavior.

A key concern while estimating the impact of a subscription program on purchase behavior arises from the lack of random assignment. We exploit the panel structure of our data

³For instance, Movie Pass, a subscription service that offered its members one free movie each day for \$9.95 per month, managed to attract more than 2 million subscribers but failed to build a deeper relationship with customers. The company reported a loss of \$266 million in 2018 and ended the service in September 2019.

and rich information on customer characteristics and rely on a quasi-experimental design to control for self-selection and identify the effect at the individual level. Specifically, our baseline model uses a difference-in-differences (DD) specification (Angrist and Pischke 2008) that controls for unobserved individual-fixed and time-fixed effects to estimate the treatment effect on customer behavior. In addition, to enhance the comparability between members and non-members, we create a weighted set of neighboring observations for each member based on a large set of observed characteristics following a generalized random forests (GRF) procedure (Athey et al. 2019) and estimate the DD model using the weighted sample. The combination of the DD approach and GRF procedure is robust to selection bias based on observed as well as time-invariant unobserved characteristics. It also provides individual-level estimates of the treatment effect. Within this framework, we also quantify the non-economic effect by evaluating the residual effect of the program after controlling for the marketing mix a member was exposed to. The individual-level estimates of economic and non-economic effects allow us to get a richer understanding of the impact of the subscription program and its underlying drivers.

We find subscription is effective in lifting sales. On average, members increased their purchases by about \$27 per month over a 12-month period post subscription, which is more than double of customer monthly purchase amount prior to subscription. The treatment effect of subscription is economically significant and persistent over time. The subscription keeps members more engaged in terms of frequency and variety in their purchases. Interestingly, only one-third of the treatment effect on purchase amount is due to the economic benefits of the program and the remaining two-thirds is attributed to the non-economic effect. There is also a large variation in the treatment effect across customers. Our main findings are robust to potential confounding effects of self-selection and unobservables, different treated groups, and different outcomes of purchase behavior.

We investigate the potential drivers at work that explain our findings. We find that, in addition to the psychological underpinnings (e.g., habit, status, affect) documented in the

context of other types of membership programs (e.g., loyalty programs), a unique feature of subscription programs helps sales lift: as customers pay a fee upfront in exchange for future benefits, they experience sunk cost fallacy in which they increase their purchases to justify their subscription decisions, even though the upfront fee is sunk (e.g., Thaler 1980, Arkes and Blumer 1985). We provide evidence supporting this mechanism. Further, we provide the profitability of the subscription program.

Our paper is related to several streams of research. First, we contribute to the literature on subscription business. McCarthy et al. (2017) develop a framework for valuing subscription-based firms and Datta et al. (2017) study how the adoption of music streaming subscription affects listening behavior. Our paper makes both substantive and theoretical contributions to this nascent literature. Substantively, existing literature focuses on replenishment and curation subscriptions (e.g., McCarthy et al. 2017) which are in the form of standalone services. Our research extends this literature by studying a program initiated by an existing non-contractual business. Theoretically, we add to the literature that studies the underpinnings of subscription programs. We document a novel mechanism through which subscription programs can work (i.e., sunk cost fallacy). Because customers pay a fee upfront, they increase their purchases to take advantage of program benefits to justify their subscription decisions. As the upfront fee is a feature common to subscription programs, we believe our findings have broad implications that the effect of a subscription program can indeed go beyond the economic benefits it offers.

We also add to the literature on membership programs. Firms across a wide array of industries have long been using loyalty programs to reward repeat purchases, and there is extensive research on these types of programs. Some studies find loyalty programs can increase customer lifetime value and share of wallet (e.g., Lal and Bell 2003, Liu 2007, Kopalle et al. 2012, Gopalakrishnan et al. 2020). Others find no or weak evidence loyalty programs are effective (e.g., Hartmann and Viard 2008). Several researchers have documented loyalty programs can lead to the development of habitual consumption (e.g., Wood and Neal

2009), enhance members' perception of status (e.g., Drèze and Nunes 2009), and induce positive affect (e.g., Leenheer et al. 2007). This paper contributes to this literature by using quasi-experimental data to measure the effect of subscription on customer behavior.

Our findings suggest that customers behave in a boundedly rational manner, which adds to empirical evidence for such behavior found in the lab and in other field settings. The sunk cost fallacy has implications in a variety of contexts and its extensive evidence has been found in the lab (Thaler 1980). There are relatively few studies, however, providing evidence for the sunk cost fallacy in the field. For example, Arkes and Blumer (1985) conduct a field experiment and find the attendance rate is positively correlated with the price of theater tickets. Ho et al. (2017) find evidence for the sunk cost fallacy in the Singapore automobile markets where there is heterogeneity among consumers with regard to the payment for obtaining a government license to purchase a car. And the driving time is positively correlated with the price paid. We extend this literature by showing that subscription programs also induce the sunk cost fallacy and contribute to our understanding of consumer behavior in the field using observational data.

The remainder of the paper is organized as follows. Section 4.2 gives an overview of subscription business. Sections 4.3 and 4.4 discuss the data and methodology. Section 4.5 presents the results and discusses possible explanations. Section 4.6 presents several robustness checks. Section 4.7 discusses the profitability of the subscription program. We conclude in Section 4.8.

4.2. Subscription Business

A subscription-based business is one in which a customer pays a fee to have access to products or services. Rather than selling products one at a time, a subscription offers periodic (e.g., monthly, yearly) use or access to products or services. Thus, a one-time purchase of a subscription can lead to recurring sales and a predictable stream of revenues from subscribers. Pioneered by the likes of newspapers and magazines, more products and services are being offered through subscriptions than ever before. For instance, business-to-

consumer subscription businesses attracted more than 11 million subscribers in 2017 in the U.S. and the industry as a whole has been growing at a staggering rate of 200% annually since 2011.⁴

Despite sharing a common feature of offering the use of or access to products or services for a fee, subscription-based businesses appear in many different formats. Existing business-to-consumer subscription business in retail can be broadly categorized into three types: replenishment, curation, and access.⁵

Replenishment subscriptions allow consumers to automate the purchase of commodity items, such as razors, diapers, and vitamins. Customers benefit from this type of subscription because it allows them to save time and money on each transaction. Examples include Dollar Shave Club, Gillette on Demand, and Rituals. Curation subscriptions seek to delight by providing new items or personalized experiences in such categories as apparel, beauty, and food. Examples include Stitch Fix, Birchbox, and Blue Apron.

In this paper, we focus on the third type of subscriptions, access subscriptions, which allow consumers to gain exclusive access or member-exclusive benefits. Access subscriptions have attracted substantial interest among more established retailers, as compared to the first two types of subscriptions which are mostly launched by start-ups. Examples include Amazon (Prime), Barnes & Noble (B&N Membership), Sephora (Flash), Alibaba (88VIP), etc. Access subscriptions differ from the other two described above in that a subscription program is initiated by an existing non-contractual business and provides members exclusive benefits beyond those available to regular customers (i.e., non-members). Thus, access subscriptions have a very wide appeal, as they can be adopted by nearly all business-to-consumer firms. While there are many variations of such programs in practice, benefits

⁴“Subscription Business Are Booming. Here’s How to Value Them,” available at <https://hbr.org/2017/12/subscription-businesses-are-booming-heres-how-to-value-them> (accessed April 5, 2021).

⁵As subscription-based businesses continue to grow, we recognize that the three categories described may not be sufficient to capture the diversity. See “Thinking Inside the Subscription Box: New Research on E-commerce Consumers,” available at <https://www.mckinsey.com/industries/high-tech/our-insights/thinking-inside-the-subscription-box-new-research-on-ecommerce-consumers> (accessed April 5, 2021).

offered to members typically fall into two broad categories: unlimited use of a service (e.g., free shipping) and access to member-exclusive offers. Amazon (Prime) and Barnes & Noble (B&N Membership), for example, offer both types of benefits to their members while Sephora’s Flash offers only unlimited free shipping. Firms also vary by the type of member-exclusive offers. Barnes & Noble provides members with exclusive offers only for purchasing products, while Amazon and Alibaba (88VIP) offer exclusive digital content to their members as well as member-only benefits related with product purchases. For the remainder of the paper, we refer to access subscriptions as subscriptions for brevity and use the terms subscription program and program interchangeably.

While there is anecdotal evidence suggesting the commercial success of subscriptions, no study has evaluated whether they indeed lead to incremental revenues from members as compared to non-members, who can purchase from a firm without being subscribers. It is also unclear whether the subscription program is effective in inducing customers to change their subsequent behavior because of the non-economic benefit and/or the economic benefits associated with the program. For the former, mere membership can bring value to customers and influence their behavior. For instance, membership to an exclusive country club can bestow status and change purchase patterns.⁶ For the latter, as discussed above, subscription programs usually come with some form of economic benefits. Our research aims to fill in this gap.

4.3. Empirical Context and Data

We obtained the data for our empirical analysis from a retailer in Asia. The retailer sells a wide range of beauty products (e.g., skin care, make-up). It has significant brick-and-mortar and online presence, with the latter being smaller than the former. In December 2015, a subscription program was launched on its e-commerce website. The launch of the program and its benefits were communicated through mass emails and on their website, and no targeting was involved. The program provided both unlimited use of a service and access

⁶“The In Crowd: Inside Boston’s Elite Country Clubs,” available at <https://www.bostonmagazine.com/news/2018/09/11/in-crowd-country-clubs/> (accessed April 5, 2021).

to member-exclusive offers. Specifically, for a subscription fee of \$50 per year, members had access to unlimited free shipping with no minimum purchase requirements. Members also had access to several exclusive offers. Upon joining the program, customers were provided with a \$50 gift card that could be used for purchases online with no restrictions. Members also received a \$3 gift card per month for online purchases during the month. Free samples were offered to members monthly with a purchase online. Finally, several products were occasionally coupled with member-exclusive discounts. Similar to other subscription programs, these benefits were offered beyond the first year of subscription rollout. Other than the program benefits mentioned above, there is no difference in the communication between members and non-members during the data period.

Our data include 10,811 customers who joined the subscription between December 2015 and February 2017. On average, 720 customers joined the program per month. The monthly number of members has a moderate level of variation, ranging from 342 to 1,062, with a standard deviation of 210. For the purpose of comparison, we also obtained a random sample of 13,768 customers who had yet to subscribe the program as of July 2017. The data consist of two parts: transaction data and program-usage data. The transaction data contain detailed information on each purchase made by a customer, when the customer purchased a product and how much she paid for it. The program-usage data contain information on how a member benefited from the program, e.g., amount spent with gift cards, free samples received, etc. Our data also contain socio-demographic characteristics of customers, e.g., age, gender, and address, which we utilize to control for customer heterogeneity while explaining the drivers of purchase behavior.

Using transaction data, we define a set of outcome measures associated with customer purchases. As the program was offered at the online channel only, unless specified otherwise, they are based on online purchases and are constructed at the customer-month level, which is the unit of analysis in this research. As our main interest is to assess how effective the pro-

gram is in lifting sales, our primary measure is the amount spent by a customer per month.⁷ In addition, we consider two other (monthly) measures of customer purchases—number of purchases made (purchase frequency) and basket size (\$) conditional on purchase (basket size). These two measures, while both positively correlate with the amount spent, may have differing implications for the firm in terms of customer engagement and costs. We also characterize the variety in purchase behavior with a few metrics. We classify a product (and its category) a customer purchased as a new versus known product (category) on the basis of whether she had purchased it in the pre-subscription period. The first set of metrics relates to the variety at the product level: amount spent for new versus known products. The second set of metrics relates to the variety at the category level: amount spent for new versus known categories.⁸ As a proxy for engagement to the firm, these measures are useful to investigate how customers change their behavior post subscription.

4.4. Method

In this section, we first discuss the empirical strategy and treated and control groups to establish the causal effect of the subscription program. We next discuss the difference-in-differences approach and generalized random forests procedure followed by implementation details.

4.4.1. Empirical Strategy

A key challenge in identifying the impact of subscription on purchase behavior is due to self-selection—members may differ from non-members even before they subscribe the program and this may lead to biased results if we estimate the effect by directly comparing purchases between members and non-members. We also seek to identify the economic and non-economic effects of subscription on customer behavior separately. The former captures the

⁷All transactions were recorded in the currency of the country in which the headquarters of the company was located. We converted purchase amount to U.S. dollars using the average exchange rate over the data period.

⁸Based on conversations with the retail partner, we decided to have five product categories for our empirical analysis to correspond to the way in which the firm monitors key metrics regarding customer purchases. They include skincare, make-up, hair care, bath and body care, and others in which we aggregated and grouped fragrance and the rest of the categories (e.g., tools and brushes).

changes in purchase behavior attributable to the tangible benefits of the program (e.g., reduced prices due to member-exclusive discounts) and the latter includes any remaining effect on demand. Finally, we are interested in examining the heterogeneity in the treatment effect across customers.

To control for self-selection and identify the effect of subscription on purchase behavior at the individual level, we rely on a quasi-experimental design. Our baseline model uses a difference-in-differences (DD) approach (Angrist and Pischke 2008) and controls for selection based on time-invariant unobservables. Within the regression framework, we separately identify the non-economic effect on purchase behavior by evaluating the residual effect of the program after controlling for the marketing mix a member was exposed to. Conceptually our framework is similar in spirit to past work that has examined how the different components of a pricing scheme may have an impact on demand over and above its economic effects (e.g., Bertini and Wathieu 2008, Iyengar et al. 2011). We complete our modeling framework by embedding the DD specification within a generalized random forests procedure (Athey et al. 2019). Briefly, we estimate the DD specification for each member using a subsample of comparable customers defined by a random forest in a high-dimension covariate space. In doing so, we account for selection based on observables and heterogeneity across customers in a non-parametric manner and obtain individual-level treatment effects.

4.4.2. Treated and Control Groups

We focus on a cohort of members who joined the program around the same time in our main analysis. Such a cohort-level analysis is common when analyzing customer value (e.g., McCarthy et al. 2017). Focusing on a cohort of members is conducive to examining the effect of the subscription program, as it gives well-defined pre- and post-treatment periods for the analysis. Our main findings consider the cohort of 721 members who joined the program in April 2016, four months after the launch of the program.⁹ As reported in Section 4.6, our findings are robust when we estimate the effect of the program among

⁹In order to mitigate the concerns for selection and unmeasured confounders, we deliberately excluded early subscribers as they may systematically differ from other customers (e.g., Rogers 2003).

members who joined in other months.

Before we establish the effect of the program on customer purchases, we examine the purchase amount for members and non-members over a 24-month period: April 2015 to March 2017. Of these, the first 12 months (April 2015 to March 2016) are prior to their subscription. Figure 10 offers model-free evidence that purchase behavior differed considerably between members and non-members, which also persisted over time. On average, members spent \$43.16 per month post subscription while non-members spent only \$3.93.

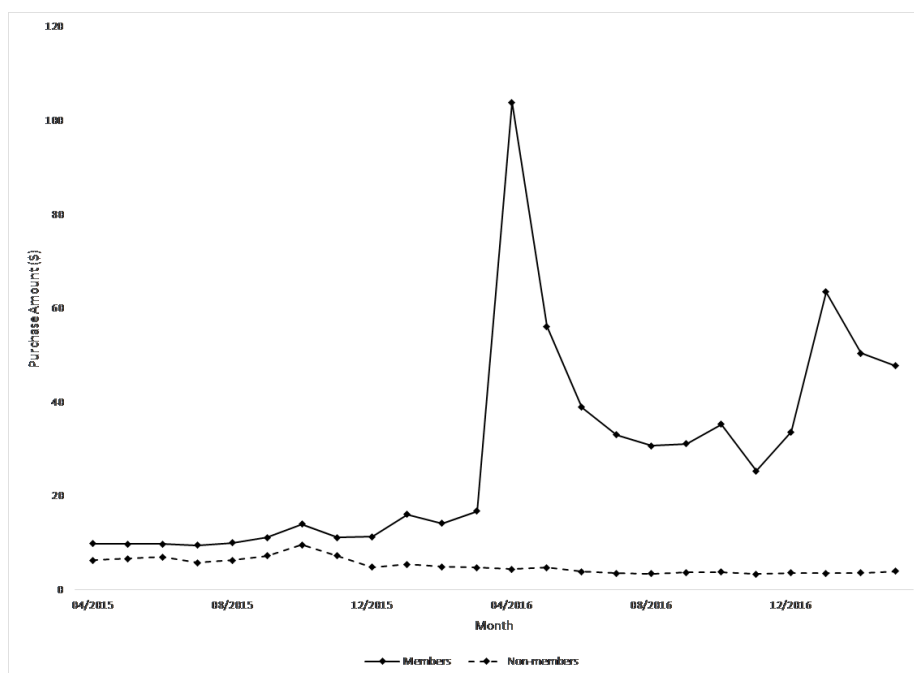


Figure 10: Customer Purchase of Members verses Non-members

To assess whether non-members were similar to members before joining the program, we compare them on their purchases during the 12-month period prior to subscription and their individual characteristics. Table 15 shows that members and non-members differed significantly on both their purchases and demographics. On average, members spent more per month than non-members (diff. = 5.61, $p < 0.001$), which is consistent with the intuition that customers who spent more were more likely to join the program as they could benefit more from the program. Members were older than non-members (diff. = 3.10,

$p < 0.001$). Clearly, estimating the effect of subscription by merely comparing customer purchases between the two groups will be biased.

Table 15: Summary Statistics of Members versus Non-members

Variable	Members	Non-members	Difference	p -value
Purchase amount (\$)				
04/2015	9.86	6.36	3.50	0.000
05/2015	9.77	6.68	3.09	0.000
06/2015	9.81	7.03	2.78	0.000
07/2015	9.54	5.84	3.70	0.000
08/2015	10.05	6.33	3.72	0.000
09/2015	11.17	7.25	3.92	0.000
10/2015	14.02	9.63	4.40	0.000
11/2015	11.18	7.27	3.90	0.000
12/2015	11.33	4.90	6.43	0.000
01/2016	16.08	5.50	10.57	0.000
02/2016	14.21	4.93	9.20	0.000
03/2016	16.81	4.77	12.05	0.000
Demographics				
Age	35.94	32.84	3.09	0.000
Gender (Female = 1)	0.93	0.94	-0.01	0.162
Observations	721	13,768		

4.4.3. Difference in Differences

The descriptive analysis reveals that members purchased considerably more than non-members post subscription. However, as discussed, the analysis may suffer from self-selection. In this section, we proceed with a DD approach which controls for time-invariant unobserved variables and quantify the (causal) treatment effect of the subscription program. To appropriately measure the non-economic effect of the program, we also control for the marketing mix that a member was exposed to. We estimate the following DD model:

$$Y_{it} = \tau \text{Member}_{it} + Z_{it}\beta + \alpha_i + \gamma_t + \epsilon_{it} \quad (4.1)$$

where Y_{it} is the outcome measure, such as purchase amount, of customer i in month t , and Member_{it} equals one if customer i was a member in month t and zero otherwise. The two

parameters α_i and γ_t are customer- and month-level fixed effects, respectively and ϵ_{it} is the error term. By including the two-way fixed effects, we control for time-invariant customer characteristics as well as common time trends and month-to-month fluctuations. The vector Z_{it} contains any marketing mix customer i was exposed to in month t and the vector β contains the respective sensitivity parameters. Of primary interest is the parameter τ , which estimates the non-economic effect of the program on purchase behavior, after controlling for marketing mix. Note that if marketing mix were not accounted for in Equation (4.1), the parameter τ would then capture the treatment effect (sum of economic and non-economic effects) on purchase behavior.

In our context, members received unlimited free shipping service and member-exclusive offers, including free product samples, additional price discounts, and monthly gift cards. We find free shipping and samples had limited impact.¹⁰ Instead, members benefited from price discounts and gift cards: members had an average 6% additional discount at the store-level as compared to non-members and about 20% of monthly gift cards were redeemed. Therefore, the vector Z_{it} includes these two marketing variables.

To model the (economic) effect of price discounts and gift cards, consider a customer who determines her purchase behavior given the store-level price and her monthly budget, which may be altered by gift cards. We specify that the customer's purchase is linear in the logarithm of the price and the logarithm of her monthly budget. The log-linear demand model has been used widely in the marketing literature to characterize consumption patterns, e.g., how customer purchases vary by income and wealth (Dubé et al. 2018). Specifically,

$$Y_{it} = \tau \text{Member}_{it} + \beta_1 \log(\text{Price}_{it}) + \beta_2 \log(\text{Baseline}_i + \text{Giftcard}_{it}) + \alpha_i + \gamma_t + \epsilon_{it}, \quad (4.2)$$

where Price_{it} is the store-level price for customer i in month t , Baseline_i is the baseline

¹⁰The firm offered customers, regardless of subscription, free shipping on orders above a certain threshold, which was satisfied by most orders. We show in Section 4.5 that the effect of free shipping on purchase is negligible after accounting for other types of economic benefits. We also find less than 1% of purchases were induced by free samples, when we examined whether a customer purchased a product after receiving a free sample of that product.

budget of customer i in absence of gift cards, and Giftcard_{it} is the amount of gift cards customer i received in month t . The parameter τ captures the non-economic effect. The parameters β_1 and β_2 are the semi-elasticities of price and gift card, respectively. This demand system is consistent with utility maximization (e.g., Sato 1972).¹¹

We compute the store-level price for customer i in month t (Price_{it}) as the weighted average price for a basket of products (e.g., Dubé et al. 2018). Given that we do not observe the prices of all products that a specific customer was exposed to, we proxy the customer-level prices using group-level (members and non-members) prices. This assumption is reasonable as there was no other source of price discrimination between members and non-members beyond the discounts from subscription. Specifically, we operationalize the monthly store-level price as the weighted average price of a basket consisting of all products purchased by both members and non-members in that month.¹² Additionally, we operationalize the baseline budget of a customer (without gift cards) by using her maximum monthly spend one year prior to subscription.

The identification of Equation (4.2) exploits the panel structure of our data and heterogeneity in the baseline budget. Our panel consists of data over a 24-month period, where the first 12 months are prior to subscription. We excluded customer purchases in the month of adoption (April 2016) to avoid any potential simultaneity bias with the adoption itself. We use the month-to-month variation in store-level price to identify the price coefficient (β_1). Note that all members received a gift card worth three dollars each month. The parameter β_2 is identified by the difference of the unexplained change in purchases upon subscription across members who have different baseline budgets. For example, a positive β_2 indicates the increase in customer purchases declines as the baseline spend increases. The parameter τ is identified by the remaining change in member purchases upon subscription and provides

¹¹Appendix B.3 discusses conditions under which our demand model is consistent with utility maximization.

¹²Appendix B.2 describes how store-level price is operationalized for the main analysis and discusses the robustness of our results under alternative operationalizations.

an estimate for the non-economic effect of the program.¹³

4.4.4. Generalized Random Forests

While the DD model controls for time-invariant heterogeneity and estimates the average treatment effect, there are two issues worth discussing. First, note from Figure 10 and Table 15 that the difference in purchase amount between the two groups widens over time, suggesting that compared to an average non-member, the monthly spend by an average member increased over time even before her subscription. Thus, the parallel time trend assumption may not hold and the validity of the DD estimator is questionable (e.g., Bertrand et al. 2004).¹⁴ Second, a typical way to accommodate heterogeneity in the treatment effect is to interact the treatment dummy with individual characteristics. This approach can become cumbersome as the covariate space increases and moderates the treatment effect in a non-linear manner.

Recent developments in the machine learning literature allow us to address both issues in a principled manner. We employ the generalized random forests (GRF) method (Athey et al. 2019). Similar to other methods for causal inference using observational data, e.g., kernel matching (e.g., Hastie et al. 2009), propensity score matching (e.g., Hirano et al. 2003) and synthetic control (e.g., Abadie et al. 2010), the key idea of GRF is to define for each member a weighted set of neighbors that shares similar covariates and fit the model of interest using these neighbors. As an improvement to the traditional methods where the weights are chosen by deterministic kernel functions (kernel matching), parametric models (propensity score matching) or trend matching (synthetic control), Athey et al. (2019)

¹³Appendix B.7 describes how different patterns in purchase data could help identify model parameters.

¹⁴We tested whether purchase trends are common prior to subscription. To that end, we extended the DD specification in Equation (2) and estimated the difference in purchase behavior between members and non-members in month m using the following specification: $Y_{it} = \sum_m \tau_m \cdot I(t = m) \cdot \text{Member}_i + Z_{it}\beta + \alpha_i + \gamma_t + \epsilon_{it}$, where the indicator variables $I(t = m)$ are 1 if month t is m and Member_i indicates whether customer i belongs to the treatment or control group. We normalized the first month in the pre-treatment period as the baseline of 0. Thus, the parameter τ_m captures the average difference in purchase measures between members and non-members relative to the baseline. We find the estimates are positive and statistically significant in the months close to the subscription, e.g., December 2015 to March 2016. We also estimated the model without marketing mix and find the parallel trend assumption does not hold. Results are available from the authors upon request.

propose to learn the weights using a revised random forest algorithm that is designed to minimize the estimation error.

Given the forest, we can define for each member with covariates x a weighted set of her neighboring customers by locating which customers fall into the leaves that contain the same covariates and associated frequency. The treatment effect for this member is estimated by fitting the DD specification on the weighted set. As compared to other commonly used methods for matching, GRF is non-parametric and robust to model misspecification. The tree structure and the ensemble of many trees naturally account for complex interactions among covariates. The adaptive nature in trees can substantially increase the accuracy of the weighting function with a large space of covariates. Another advantage of GRF is that it uncovers the point estimates and confidence interval of the treatment effect at the individual level with formal asymptotic guarantees. These estimates allow us to explore heterogeneous treatment effects in a systematic manner and can sharpen our understanding of underlying drivers for the success of the program.

4.4.5. Implementation

For the outcome variables, we analyze transaction data over a 24-month period (April 2015 to March 2017) because we are interested in examining the effect of subscription for the long term. Of these, the first 12 months are prior to subscription. As noted earlier, we excluded customer purchases in the month of subscription adoption (April 2016).¹⁵

To control for potential confoundedness, we include three sets of covariates that describe members and non-members in the pre-treatment period. The first set of covariates relates to the customer-firm relationship, which would be associated with the adoption of a service, namely, tenure, breadth, and depth (e.g., Bolton et al. 2004, Prins and Verhoef 2007). We calculate tenure based on elapsed time since having an account on the website. We measure breadth by the number of unique categories purchased and depth by the number of

¹⁵The estimate we report provides a conservative estimate of the effect of subscription on customer purchases. Detailed results, which include the month of subscription adoption, are available from the authors upon request.

transactions made. We also include the average basket size. In addition, we include monthly purchase amount per category during the 12-month pre-subscription period, instead of the total amount across product categories, because it could help find clusters of customers with similar purchase patterns across categories. We also include the standard deviation of monthly purchase amount because it could relate to customer response for unlimited free shipping service.

The second set of covariates relates to psychographic measures that reflect personality traits, preferences or interests, values and attitudes (e.g., Baumgartner 2002). As psychographics are not directly observable, their measures are more nuanced. Given that we use observational data with no surveys sent to customers, we explore a few measures by summarizing certain aspects of purchase behavior: exploratory (purchases of new products), repetitive (repeat purchases of a product), and promotional (discounts received for purchases).¹⁶

The third set of covariates relates to socio-demographics of customers. We include age and gender. We also include the coordinates of a customer’s mailing address because it can help control for other unobserved socio-demographics that affect subscription, e.g., education, income, lifestyle, and so on. Table 16 summarizes the covariates and describes how the variables are operationalized. Altogether, we use 72 covariates to build the trees for the random forest.¹⁷

We briefly describe the procedure for building the forest. To build the random forest¹⁹, we first grow a decision tree by iteratively partitioning the data into subgroups (leaves). In

¹⁶We also conducted our analysis with alternative operationalization of the variables, e.g., share of repeat product purchases per month and category. We find the results are largely similar.

¹⁷Before we built GRF for matching, as an important first step to ensure the final estimates are credible and robust, we pre-processed data by excluding customers with extreme propensity to adopt the program and improved overlap in covariate distributions (Imbens and Rubin 2015). We estimated the propensity scores by predicting membership with the covariates using a regression forest. We find 50 of the members had propensity scores with no counterparts in the control group. We thus excluded those customers whose propensity scores did not lie on the common support. Our final estimation was conducted on a sample of 671 members and 11,745 non-members.

¹⁸We have used alternative thresholds to construct the exploratory and repetitive psychographic metrics. We find our results are robust.

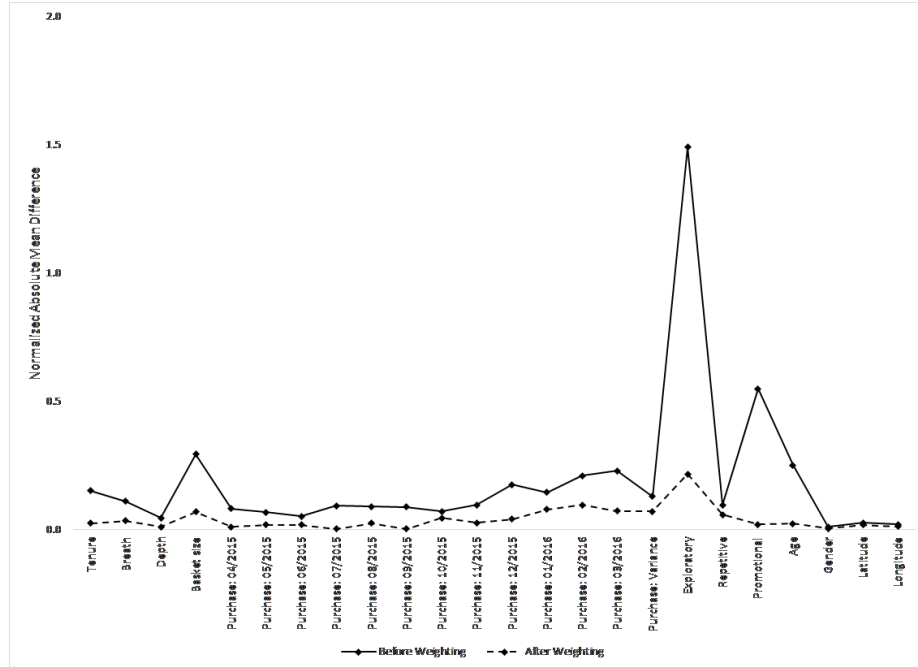
¹⁹Appendix B.4 provides the implementation details.

Table 16: Covariates for Generalized Random Forests.

Covariates	Operationalization	Mean	Std. Dev.
Customer-firm relationship			
Tenure	Elapsed time (year) since having on-line account	5.55	3.19
Breadth	Number of unique categories purchased	1.97	1.23
Depth	Number of transactions made	1.75	1.27
Basket size	Average basket size	33.51	31.26
Monthly purchase amount	Monthly spend at each product category	1.33	5.91
Monthly purchase S.D.	Standard deviation of monthly purchase amount	15.07	20.77
Psychographics¹⁸			
Exploratory	Inverse of average time (year) taken for the purchase among three new products purchased since the launch	3.12	13.39
Repetitive	1 if a customer made repeat purchases of a product more than ten times, 0 otherwise	0.03	0.18
Promotional	Average discount rate received for purchases	0.31	0.23
Socio-demographics			
Age		33.96	8.96
Gender	0 = Male, 1 = Female	0.94	0.23
Address	Coordinates of home address		

each iteration, the algorithm chooses the covariate and the cutoff to find partitions where treatment effects most differ. As a single tree is likely to overfit the data, an ensemble of trees is generated. For each tree, a random set of the covariates can be potentially used to form the splits. The number of trees, minimum number of treatment and control observations in each leaf, subsample size and size of the set of covariates used to build each tree are the hyper-parameters of a forest, which we choose by cross-validation. In the context of panel data, we also account for clusters at the individual level in the sampling as well as estimation process.

The forest performs well in balancing members and non-members. Following Imbens and Rubin (2015), we use normalized absolute mean difference to assess the degree of balance of the observables. Figure 11 shows the normalized mean difference of the variables before



and after the adjustments by GRF.²⁰ After the adjustment, the normalized absolute mean difference of the variables between the members and the matched non-members are mostly below 0.1. And the members and non-members are indistinguishable in terms of their observed characteristics.²¹

Finally, given the forest, we define for each member a weighted set of her neighboring customers, which may include both non-members and other members. For a member with covariates x , the weights are the frequency with which each customer falls into the leaves that contain x .²² The procedure then fits Equation (4.2) with the weighted set of observations for each member. Hence, we effectively combine GRF with DD specification after controlling for economic benefits in the subscription program. To obtain the treatment effect across

²⁰Covariate Balance before and after Weighting. Notes: For the purpose of illustration, we plot the normalized absolute mean difference of monthly purchase amount along with other observed characteristics. In the matching procedure, however, monthly purchase amount from all product categories were used.

²¹In Appendix B.1, we tested whether parallel time trends hold in our context using a series of placebo tests. We find members and matched non-members have statistically comparable pre-treatment time trend.

²²We used out-of-bag predictions to avoid over-fitting, i.e., only trees that do not include the member during tree building were used to produce the weights.

members, we construct a doubly robust average treatment-effect estimator by augmenting the naïve plugin estimator with a residual-based correction. The doubly robust estimator combines results from GRF and a regression-based prediction such that it is robust to mis-specification of either the matching model or regression model (Chernozhukov et al. 2018).²³

4.4.6. Findings

In this section, we discuss the main findings on the treatment effect of subscription on customer purchases and the non-economic effect. We also discuss how these effects vary over time and across customers. Finally, we explore some possible explanations underlying the effect.

4.4.7. Average Treatment Effects

Table 17 reports the average treatment effect of subscription on customer purchases.²⁴ Because our objective is to identify the effect of subscription for the long term, we first discuss the estimates over a 12-month period post subscription. The first column in Table 17 shows that on average, purchase amount per month among members increased by \$27.45. The effect is economically significant, as purchase amount per month was about \$12 prior to subscription.

The treatment effect on purchase amount is quite striking. As shown in Figure 10, however, data patterns suggest that our finding is not an artifact. We conducted a literature review and observe the impact of membership programs has substantial variation, ranging from no effect to as high as 150% increase in customer response across various empirical settings.²⁵ For instance, using an application of “a buy ten get one free program” offered by a golf course, Hartmann and Viard (2008) find no changes in customer response. Similarly, Lewis

²³Appendix B.8 describes how the individual-level estimates are identified.

²⁴Appendix B.9 reports the results in which outcome measures are transformed with natural log in the estimation. The results are qualitatively similar

²⁵Lal and Bell (2003) examine the impact of frequent shopper programs in grocery retailing and find an increase of \$98, \$141, and \$150 across three different segments, respectively. As they do not report the baseline prior to the frequent shopper program in their study, however, we are not able to compute the relative impact of the program.

Table 17: Treatment Effects

	All months	Month 2	Months 3-4	Months 5+
Purchase amount (\$)	27.45*** (1.75)	40.75*** (4.70)	28.95*** (2.33)	25.53*** (2.03)
Purchase frequency	1.15*** (0.05)	1.37*** (0.02)	1.29*** (0.03)	1.10*** (0.01)
Basket size (\$)	-5.14** (2.47)	-9.44* (5.23)	-5.01*** (1.76)	-5.92*** (2.02)
Variety (\$)				
Purchase amount in known categories	26.19*** (1.77)	40.62*** (4.83)	27.22*** (2.30)	23.61*** (2.07)
Purchase amount in new categories	1.99*** (0.29)	1.00*** (0.32)	2.64*** (0.57)	1.77*** (0.27)
Purchase amount in known products	7.13*** (0.68)	6.02*** (3.05)	7.65*** (0.84)	7.18*** (0.73)
Purchase amount in new products	20.89*** (1.67)	35.74*** (3.26)	22.22*** (2.19)	17.86*** (1.93)

Notes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses. Customer purchases in the month of adoption (month 1) were excluded in the analysis.

(2004) evaluates a loyalty program from an online merchant and finds about 2% increase in customer revenue. Using data from a men’s hair-salon chain, Gopalakrishnan et al. (2020) find the introduction of a non-tiered loyalty program increases customer value by 19% over a five-year horizon. Kopalle et al. (2012) use data from the loyalty program of a hotel chain and find about a 30% increase in customer spending due to the program. Using data from a convenience store chain’s loyalty program, Liu (2007) finds consumers whose initial patronage levels were low or moderate considerably increase their spending by around 150% under the loyalty program.

We note that while both Hartmann and Viard (2008), Kopalle et al. (2012), and Gopalakrishnan et al. (2020) study customer response to reward programs in a single category (e.g., hotel), Liu (2007) examines the impact of a loyalty program on customer behavior in a firm offering multiple categories. Our empirical context is close to Liu’s because the focal firm offers a wide assortment of brands and products across different categories. The effect size

in our study is considerably larger than those documented in the literature on (free) loyalty programs. Our finding is an important addition to the literature because our study examines customer response in a contractual subscription program where a one-time purchase of a subscription can lead to recurring sales. Subscribers are more likely to make repeat purchases during their subscription. In contrast, existing literature examines customer response in non-contractual reward programs.

The increase in customer purchases could be driven by the increase in purchase frequency and/or basket size. Our results show members made about one additional purchase per month (1.15) post subscription. Interestingly, we find a small but significant decrease in basket size (-\$5.14). One possibility is that members might make their basket into smaller ones to utilize, for example, recurring monthly gift cards. Our results on the economic effects of subscription later confirm this intuition.

We next examine the effect of subscription on the variety in purchase behavior. Recall that we classify products (categories) a customer purchased to new versus known products (categories) based on prior purchase behavior. We find a significant increase across all variety measures.²⁶ At the category level, more than 95% of the increase in purchase amount (\$26.19 out of \$27.45) came from known categories. At the product level, approximately 75% of the increase in purchase amount (\$20.89 out of \$27.45) was from new products that a customer had never purchased. Taken together, our evidence supports that subscription makes members purchase more frequently, with a greater variety of products and categories, leading to increased customer loyalty and share of wallet.

Finally, we examine the temporal variation in the treatment effect on purchase behavior. For instance, the program could create an initial excitement among members, leading to increased purchase. If the novelty effect of the program were the only underlying reason

²⁶We note that the treatment effect on purchase amount is slightly different from the sum of the treatment effect on purchase amount in known versus new products and categories. Because our matching procedure minimizes the mean squared error, matches depend on both covariates and outcome variable. Therefore, matches can differ slightly when evaluating the effect on different outcome variables. However, unconfoundedness guarantees that all the estimates are unbiased.

for the behavioral change, the effect will likely fade away over time and the program would have limited impact on the firm’s long-term revenue (e.g., Galak and Redden 2018). To examine the temporal effect of subscription on purchase behavior, we utilize the purchase patterns shown in Figure 10 and distinguish between the treatment effects within the first two months (excluding the first month of adoption), the next two months and the remaining months in the post-subscription period. We estimate the temporal effects by applying GRF on data in the corresponding time periods relative to the 12-month pre-treatment period. We find the effect of subscription on customer purchases is the largest (an increase of \$40.57) within the first two months and persists (an increase of \$25.53 per month) after four months upon subscription.

In summary, there is a causal impact of subscription on customer purchases. The effect is economically and managerially significant and is persistent over time. The program keeps customers more engaged both in terms of frequency and variety in their purchases.

4.4.8. Non-economic Effects

We next discuss the non-economic effect of subscription after the impact of the tangible benefits of the program is controlled for. Table 18 reports the average non-economic effect of the program. Interestingly, we find about two-thirds of the treatment effect on purchase amount (\$17.91 out of \$27.45) is due to the non-economic effect. By calculating the relative contribution of marketing mix (price discounts and gift cards), we find price discounts accounted for about \$4.20 increase in purchases and gift cards explained about \$5.30 increase in purchases.²⁷

Looking at the temporal patterns of the non-economic effect, we find the increase in purchase amount is largest (an increase of \$37.28) within the first two months post subscription.

²⁷We checked the face validity by comparing our price and coupon elasticities to those documented in past research. We find the price elasticity (percentage change in purchase amount in response to a one percent change in price) is -1.75 and the marginal effect of coupon (dollar change in purchase amount in response to a one-dollar change in coupon) is 1.76. As a point of comparison, in two meta-analyses, Tellis (1988) and Bijmolt et al. (2005) report average price elasticities of -1.76 and -2.62, respectively. And Venkatesan and Farris (2012) report the marginal effect of coupons to be greater than 2, as the mere exposure to coupons can help lift revenue.

Table 18: Non-economic Effects

	All months	Month 2	Months 3-4	Months 5+
Purchase amount (\$)	17.91*** (1.96)	37.28*** (9.51)	22.26*** (2.70)	12.31*** (1.99)
Purchase frequency	0.97*** (0.04)	1.20*** (0.11)	1.24*** (0.07)	0.80*** (0.05)
Basket size (\$)	-6.19 (12.77)	-2.88 (8.89)	-4.63* (2.80)	-6.67 (10.47)
Variety (\$)				
Purchase amount in known categories	17.44*** (1.96)	35.74*** (7.22)	21.77*** (3.00)	12.36*** (2.46)
Purchase amount in new categories	1.40*** (0.23)	1.06*** (1.24)	1.44** (0.58)	1.27*** (0.16)
Purchase amount in known products	3.26** (1.29)	7.20* (3.29)	1.06 (1.93)	3.99*** (1.31)
Purchase amount in new products	14.23*** (1.42)	28.59*** (6.02)	20.47*** (2.19)	10.56*** (2.02)

Notes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses. Customer purchases in the month of adoption (month 1) were excluded.

While it gradually faded away over time, the effect is persistent and managerially important. Across most of the other metrics as well, a significant part of the increase in customer purchases is not accounted for by the economic benefits of the program. The non-economic effect on basket size is not significant, indicating that after accounting for the economic benefits, there is no further impact on the basket size. This result confirms that the effect of free shipping on purchase is negligible after accounting for other types of economic benefits.²⁸ In summary, even after controlling for the economic benefits of the program, a significant part of the effect on customer purchases can be attributed to the non-economic effect of the subscription program.

²⁸This finding also replicates when we examined whether there are changes in the number of orders below the free shipping threshold. We find members had significantly more orders below the free shipping threshold post subscription. However, after taking price discounts and gift cards into account, the effect becomes insignificant. We conclude that offering free shipping did not affect customer purchases after accounting for marketing mix.

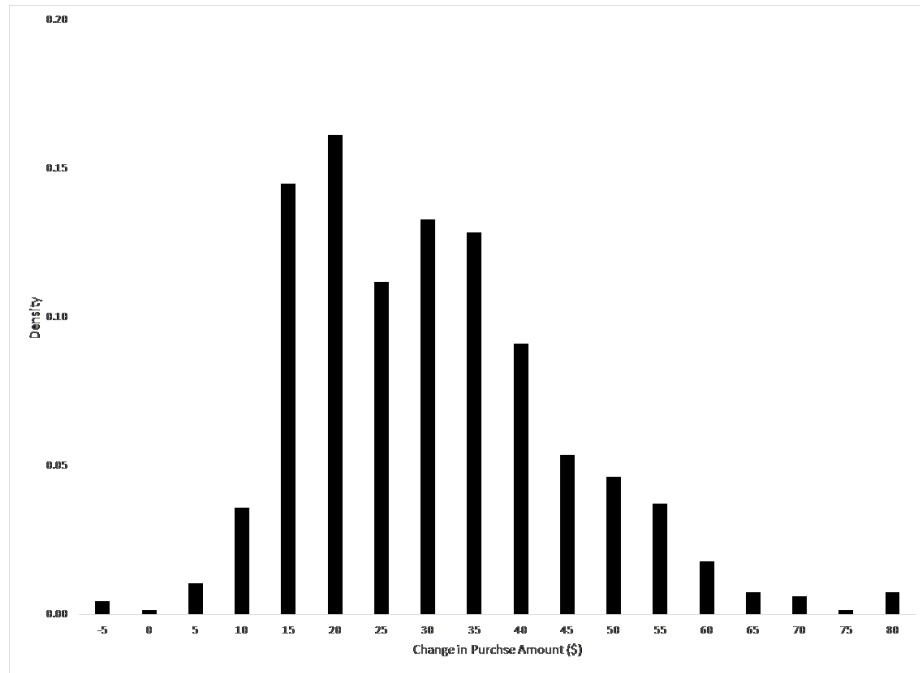
4.4.9. Heterogeneous Treatment Effects

Figures 12a and 12b show the distribution of the treatment and non-economic effects of the subscription program on purchase amount, respectively. Both figures present that the impact of subscription on customer purchases is heterogeneous across customers. These results illustrate the benefits of employing GRF, in that we can obtain individual-level treatment effects.

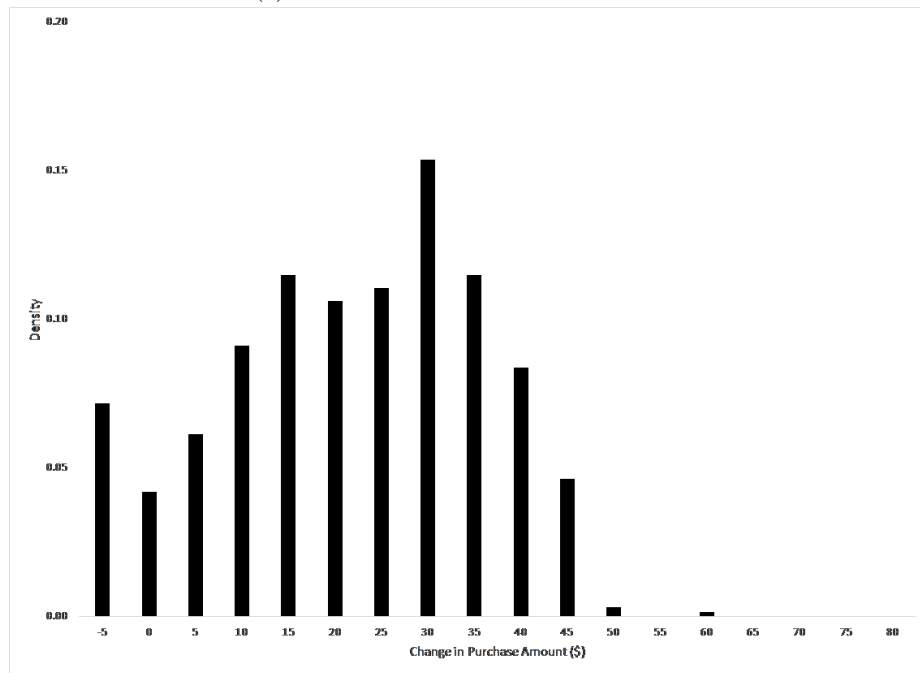
Figure 12a shows significant variation in the treatment effect across members, ranging from less than \$10 to around \$80. About 20% of the members increased their purchases by \$15 or less and approximately 27% increased their purchases by \$40 or more. Figure 12b illustrates that there is large heterogeneity in the non-economic effect as well. About 40% of the members increased their purchases by \$15 or less and around 14% increased by \$40 or more. The proportion of the treatment effect explained by the non-economic effect also has substantial heterogeneity across members. On average, 65% of the increase in purchase amount was driven by the non-economic benefit of the program. Among 15% of members, almost 90% of the increase in purchases could be attributed to the economic benefits of the program. In contrast, 40% of members would change their behavior even without economic benefits, in that more than 80% of the increases in purchase was due to the non-economic effect of the program. These results show that paid customer programs (e.g., subscription, reward) can have an impact on purchase behavior even after accounting for their economic benefits (e.g., Bolton et al. 2000).

4.4.10. Possible Explanations

We discuss four potential underlying drivers based on extant literature. The four drivers each generate unique predictions regarding the temporal purchase patterns. To facilitate a sharp test of whether these drivers are at work, we obtain more granular estimates of member-specific non-economic effects at the monthly level. Based on these member-month-level estimates, we use a unified panel regression to examine the driver(s) at work. We caution that as is typical in any research with observational data, it is hard to indicate only



(a) Distribution of the Treatment Effect



(b) Distribution of the Non-economic Effect

Figure 12: Distribution of the Treatment & Non-economic Effect

a single mechanism in explaining our documented effect (e.g., Guo et al. 2020). Our analysis thus serves as a useful first step to understand the mechanism(s) under which subscription programs may work.

Our first driver is based on a unique feature of subscription programs. Unlike members of (free) loyalty programs, members of a subscription program pay an initial fee to join the program. While rational customers should not take this fee into account when making subsequent purchase decisions, extant work suggests people exhibit sunk cost fallacy and tend to increase their purchases to justify their initial upfront payment (e.g., Thaler 1980, Arkes and Blumer 1985). We hypothesize sunk cost can be especially salient for subscription programs and induce an increase in purchase amount beyond what the economic effect of the program could explain.

We also investigate the presence of other drivers based on extant research on loyalty programs (e.g., Bolton et al. 2004). A membership program can induce a positive affect towards the firm and lead to increased purchases (e.g., Leenheer et al. 2007). Members may also feel superior to other customers when they have access to exclusive offers and their enhanced status can encourage purchases as well (e.g., Drèze and Nunes 2009). Additionally, a program may encourage customers to purchase upon joining and this increase in the short run may lead to a habitual increase in the long run (e.g., Wood and Neal 2009). While each driver is predicted to increase subsequent purchases, there are differences amongst them with respect to how some program benefits moderate their impact. Next, we discuss this aspect for each of the four drivers.

Past research on sunk cost fallacy suggests that as customers accumulate the use of a product or service after an upfront payment, sunk cost becomes less salient and its effect wears off (e.g., Ho et al. 2017). Thus, if members responded to sunk cost, their increase in purchase (after controlling for economic benefits) should decrease with how much they benefited from the program. In particular, the depth of price discounts can vary from month to month, creating variation in how members benefited from the program over time. The

prediction based on the second explanation, positive affect, is that customers act favorably towards the program shortly after they become a member and experience hedonic decline as they continue to purchase from the firm (e.g., Galak and Redden 2018). Thus, we would expect the impact of the program to decrease with past purchase amount. The presence of the third driver, status, would suggest that the effect of the program would diminish as membership becomes less exclusive. Extant research on membership programs suggests that the value of status created by membership is associated with its distinctiveness (e.g., Grier and Deshpandé 2001). If members derive status from the program, the program should have a smaller impact on their purchases as the number of members increases. The fourth driver, habit formation, would predict that a high level of customer purchase in the long term is a result of state dependence (i.e., habits formed) based on the increase in purchase in the short term. In summary, the four potential underlying drivers predict differing temporal patterns for the non-economic effect of the program and purchase behavior, allowing us to investigate the mechanism(s) at work.

We investigate the impact of all four underlying drivers on purchase behavior in a unified regression framework. To study the temporal patterns of the non-economic effect, we first obtain more granular estimates of the non-economic effect at the member-month level. Specifically, we obtain the non-economic effect for member i and month t , $\hat{\tau}_{it}^N$, by evaluating the changes in monthly purchases in month t relative to the pre-subscription period using the same procedure and covariates as in the main analysis in Section 4.4. These estimates then become the dependent measure in a panel regression, where we investigate their association with the moderators to test for the presence of the four drivers. We consider the following specification:

$$\hat{\tau}_{it}^N = \gamma_1 \cdot \text{Discount}_{t-1} + \gamma_2 \cdot \text{CumPurchase}_{i,t-1} + \gamma_3 \cdot \text{NMember}_t + \delta_i + \xi_{it}, \quad (4.3)$$

where Discount_{t-1} is the discount rate that members received in month $(t-1)$ relative to non-members and captures the presence of sunk cost. The lower the discount rate, the

smaller the sunk cost. The variable $\text{CumPurchase}_{i,t-1}$ is the cumulative amount spent by member i till month $(t - 1)$ and captures the impact of hedonic decline and other forms of state-dependent patterns. The variable NMember_t is the number of members who had joined the program till month t and is a proxy for status. We also include individual-fixed effects (δ_i) in the analysis. Finally, ξ_{it} is the error term.

By construction, the dependent and independent variables in Equation (4.3) may display systematic time trends. For example, the cumulative purchase amount and number of members likely increase monotonically over time while the non-economic effect may decline. To account for the spurious correlation between the non-economic effect and the cumulative variables due to their common time trends, we carry out first differencing (Hamilton 2020). In addition, as the dependent variable is estimated with error, we use generalized least squares to estimate the model to account for the variance of the dependent variables (Hanushek 1974).

Table 19 reports the results. The results indicate that consistent with sunk cost fallacy, the increase in purchase was negatively correlated with program benefits utilized and positively correlated with sunk cost.²⁹ We find the non-economic effect was negatively correlated with past purchase. This pattern suggests that hedonic decline with past purchase was present and habit formation was unlikely to be the main driver in our context. Finally, we find the number of members also significantly affected the non-economic effect. While we cannot interpret these results as causal, the pattern of results is consistent with the hypotheses that members increased their purchase due to positive affect towards the firm and status. Importantly, after accounting for all the other patterns, we find evidence that customers exhibited a sunk cost fallacy.

²⁹We also investigated the presence of sunk cost fallacy by examining whether members decelerated their purchases after using \$50 gift card received upon subscription. We do not find a significant decrease in purchase. Most customers used their \$50 gift card in the first month upon subscription, so there may not be enough variation to detect an effect.

Table 19: Regression Results

Construct	Variable	Estimate
Sunk cost	Discount rate	51.08*** (6.21)
Affect, Habit	Cumulative purchase (\$)	−0.055*** (0.002)
Status	Number of members	0.016*** (0.001)
Adjusted R -squared	0.152	

Notes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses.

4.5. Robustness Checks

In this section we analyze the robustness of our findings by addressing potential confounding effects of self-selection and unobservables, different treated groups, and different outcomes of purchase behavior.³⁰

4.5.1. Selection on Unobservables

The GRF framework has advantages over other causal inference methods, e.g., propensity score matching and nearest neighbor matching, in that it matches members and non-members in a non-parametric and robust manner. Since the treatment is not assigned randomly, the validity of the method still hinges on the assumption of unconfoundedness, i.e., the treatment status is not correlated with the unobservables.

While the unconfoundedness assumption is usually not directly testable, we present an additional piece of evidence to alleviate the concern for this assumption. Specifically, we use late adopters, rather than non-members, as controls for early adopters. The late adopters could display a closer resemblance to the early adopters than non-members if their adoption time is close enough (e.g., Goldfarb and Tucker 2011, Manchanda et al. 2015, Datta et al. 2017, Narang and Shankar 2019). We choose customers who joined the program between

³⁰We estimated both the treatment and non-economic effects on all outcome measures across different time windows as we did in our main analysis. As our primary interest is on the long-term effect on purchase amount, we only report the effect on purchase amount based on all months post subscription. Other results are available from the authors upon request.

August 2016 and November 2016 as the control group, allowing us to have enough customers to match from and enough time periods to estimate the effect. We find qualitatively similar results. On average, customers increased monthly purchase amount by \$31.38 (std.err. = 2.18). After accounting for the economic benefits, their monthly purchase amount increases by \$18.56 (std.err. = 1.92).

4.5.2. Alternate Treated Groups

In our main analysis, we used a single cohort of the members who joined the subscription program in April 2016. As a robustness check, in Table 20 and 21, we replicate the analysis for members who joined the program during other months. We also measure the average treatment and non-economic effects across cohorts by estimating the DD model (Equation (4.1)) on a matched sample combining all the (weighted) samples from the cohort-specific analyses. The last row in Table 20 and 21 reports the average treatment and non-economic effects across cohorts. Our results suggest that the effects of subscription on purchase amount across several cohorts are largely similar and our results are robust.

4.5.3. Alternate Outcomes

As the subscription program we study is an online-only program, our main analysis focused on customer purchases only on the website. The firm we partnered with has both brick-and-mortar and online presence and is able to link customer purchases between online and offline channels at the individual level through its reward program. We thus investigate whether the increase in online purchases through subscription was due to the channel-switching behavior to online from offline (e.g., Forman et al. 2009, Wang and Goldfarb 2017).

We perform the analysis described in Section 4.4 by replacing online purchases with the purchases combined between online and offline channels and retaining the operationalization of all covariates in Table 16. We find the treatment and non-economic effects on total purchase amount is \$26.72 (std.err. = 1.68) and \$19.41 (std.err. = 1.75), respectively, suggesting that in our context, the online and offline channels are only weak substitutes.³¹

³¹We also performed the analysis by controlling for additional covariates using offline purchases, e.g.,

Table 20: Treatment Effects across Cohorts.

Cohort	All months	Month 2	Months 3-4	Months 5+
Feb. 2016	26.53*** (2.02)	35.36*** (2.46)	31.31*** (5.52)	25.49*** (2.14)
Mar. 2016	25.68*** (2.42)	43.05*** (4.47)	26.58*** (2.88)	28.29*** (2.51)
Apr. 2016	27.45*** (1.75)	42.00*** (4.31)	28.56*** (2.37)	26.43*** (1.75)
May. 2016	26.52*** (1.68)	35.47*** (2.76)	25.60*** (1.76)	26.04*** (1.69)
Jun. 2016	22.52*** (2.00)	37.89*** (3.15)	25.05*** (3.13)	32.84*** (2.03)
Jul. 2016	26.82*** (1.39)	33.03*** (2.34)	27.74*** (1.91)	26.39*** (1.36)
Aug. 2016	35.80*** (3.95)	37.60*** (4.72)	32.34*** (4.03)	33.97*** (4.33)
Average	29.35*** (2.24)	38.77*** (3.64)	29.61*** (3.05)	28.29*** (2.87)

Notes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses. Customer purchases in the month of adoption (month 1) were excluded in the analysis.

In summary, the program is effective in lifting overall revenue for the firm in our context.

4.6. Profitability of the Program

A successful subscription program can lead to an increase in sales but may induce additional costs to the firm. As the profitability of the program will depend on whether the increase in revenue outweighs the additional cost, we provide the profitability of the program. We also investigate the characteristics of members who were most profitable. We leverage information on how members utilized program benefits and construct a cost measure to calculate the profitability. We note that some of the cost information for the profit calculation is not readily available (e.g., profit margins at the product level). Therefore, the cost measure we construct in this section is a proxy for the actual costs to the firm. Our intention is to illustrate the key tradeoffs a firm may face when launching or expanding the scale of their subscription program.

monthly purchase amount at the category level. We find the results are similar.

Table 21: Non-economic Effects across Cohorts

Cohort	All months	Month 2	Months 3-4	Months 5+
Feb. 2016	20.29*** (2.05)	33.93*** (1.29)	29.22*** (4.05)	17.22*** (1.59)
Mar. 2016	17.67*** (2.14)	32.87*** (1.38)	25.69*** (2.91)	15.03*** (1.75)
Apr. 2016	17.91*** (1.96)	37.28*** (9.51)	22.26*** (2.70)	12.31*** (1.99)
May. 2016	17.23*** (1.91)	32.24*** (1.12)	15.02*** (2.42)	15.28*** (1.64)
Jun. 2016	23.50*** (2.14)	36.94*** (3.21)	30.64*** (2.71)	19.02*** (1.86)
Jul. 2016	19.51*** (2.03)	29.03*** (4.18)	21.87*** (2.60)	16.44*** (1.80)
Aug. 2016	24.99*** (2.93)	34.22*** (1.35)	28.77*** (3.10)	22.02*** (2.69)
Average	20.99*** (2.35)	34.63*** (1.26)	24.67*** (2.98)	17.19*** (2.21)

Notes: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses. Customer purchases in the month of adoption (month 1) were excluded in the analysis.

We construct the (gross) profit as the increase in revenue less the increase in the cost of goods sold (COGS).³² We also consider three types of costs associated with the program: free shipping service, \$3 gift card per month, and monthly free samples with purchase. Based on our results, members ordered more frequently than non-members, resulting in slightly higher shipping costs for the firm. There were costs from gift cards and free samples as well. Using data on how members utilized the benefits of the program, we construct the cost measure as the increase in shipping cost (number of orders made multiplied by the shipping cost per order) and add the costs incurred through redemptions of gift cards and free samples.³³

³²We observe that COGS typically ranges from 20% to 30% of revenue in the beauty industry. In our analysis, we assume that COGS is 25% of the list price.

³³We cannot reveal shipping costs which the firm paid to delivery services companies due to the non-disclosure agreement and note that shipping costs in Asia are considerably lower than those in the US. The cost of a sample was derived from the price of the full-sized regular product assuming that cost is proportional to size.

We find that the program generated an average monthly net profit of \$13.71 per member. While the information on customer purchases beyond the first year of subscription rollout is not available, we also calculate the lifetime profit based on our estimates of the one-year treatment effects and the information on member retention. The expected lifetime net profit per member is \$366.³⁴ We find there is significant variation in net profit across members. Figure 13 plots each member based on the cost incurred through subscription and (gross) profit.

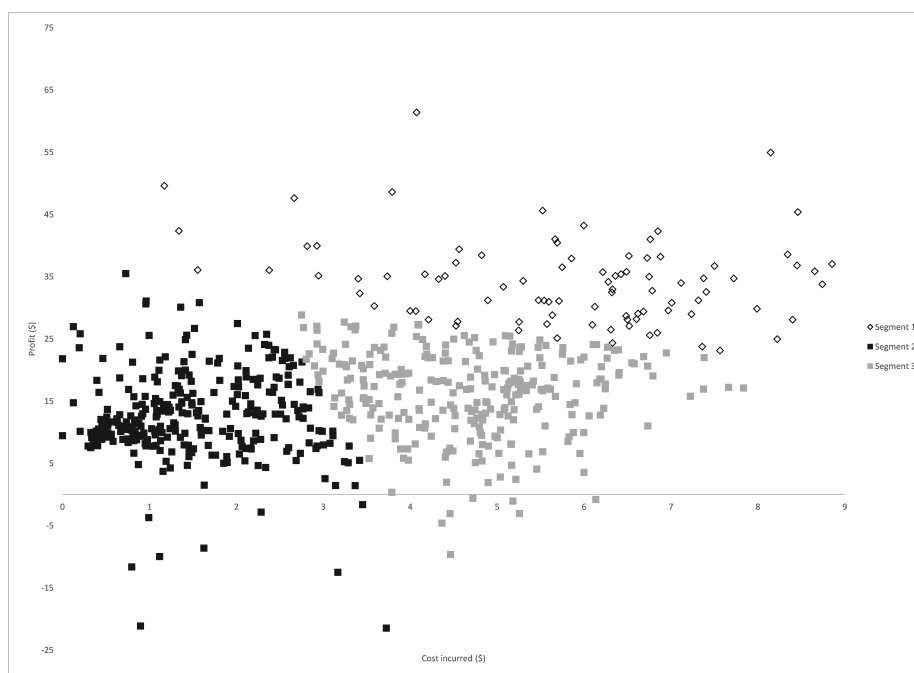


Figure 13: Scatter Plot of Profit and Cost

Taking a step further, we apply the K-means clustering algorithm to segment members based on the two measures. We find members were grouped into three segments, one small segment (segment 1 accounting for 14% of members) and two large segments (segments 2 and 3 accounting for 46% and 40% of members, respectively). Segment 1 (empty diamond in Figure 13) includes the members who contributed highest profit but incurred largest cost for the firm. This segment generated about 30% of the total profit. The largest segment,

³⁴We observe 55% of the members renewed their subscription after the first year. Assuming that the program's attrition rate stays constant over time, the expected lifetime profit per member is: Profit per year / (1 - Renewal rate) = $\$13.71 \times 12 / (1 - 0.55) = \366 .

segment 2 (black square in Figure 13) represents the members who generated a sizeable increase in profit but did not incur as much costs related with subscription. Segment 3 (grey square in Figure 13) is similar to segment 1 in terms of the incurred cost but differs significantly in terms of the profit.

We also find the three segments differed in terms of their observed characteristics. Table 22 reports the summary statistics of observed characteristics for the three segments. Interestingly, the members who contributed highest profit for the firm upon subscription (segment 1) are not the ones who purchased most prior to subscription. Rather, they were relatively less active customers in the pre-subscription period but were willing to explore new products (exploratory), had repeated purchases (repetitive), and responsive to promotions (promotional) so that they re-engaged with the firm post subscription. The high-value customers (segment 3) based on past purchase, while enjoying program benefits, increased their purchases only moderately, which is likely due to a ceiling effect. These results can assist managers in scoring and targeting future customers for the subscription program.

Table 22: Profitability and Characteristics by Segment

	Segment 1	Segment 2	Segment 3
Profit (\$)	35.21	12.53	16.16
Cost (\$)	5.84	1.52	4.70
Segment size (% of all members)	14%	46%	40%
Customer-firm relationship			
Tenure	6.46	6.02	6.37
Breadth	1.50	1.38	2.66
Depth	1.57	1.29	2.45
Basket size	13.92	26.94	29.54
Monthly purchase amount: Category-level	1.56	2.45	3.53
Monthly purchase amount: Std. Dev.	14.22	18.79	25.71
Psychographics			
Exploratory	53.09	4.03	23.41
Repetitive	0.35	0.02	0.15
Promotional	0.63	0.58	0.36
Socio-demographics			
Age	36.02	35.37	35.80
Gender	0.98	0.93	0.95

4.7. Conclusions

In this paper, we examine the causal effect of a subscription program on customer behavior. We combine the difference-in-differences approach with the generalized random forests procedure and obtain the treatment-effect estimates at the individual level. We find subscription is effective in lifting sales and keeps members more engaged in terms of frequency and variety in their purchases. Interestingly, only one-third of the effect on purchase amount is attributed to the economic benefits of the program. The effect is economically significant, persistent over time, and heterogeneous across customers. Our main findings are robust to potential confounding effects of self-selection and unobservables, different treated groups, and different outcomes of purchase behavior.

To uncover the underlying mechanism that leads to the behavioral changes, we leverage the individual- and month-level treatment effects in our estimation. In addition to multiple drivers (e.g., habit, status, affect) found in the context of other types of membership programs (e.g., loyalty programs), notably we document evidence that members experience a sunk cost fallacy due to the upfront payment that subscription programs entail. Further, we discuss the profitability of the subscription program.

Our results shed light on the practice of subscription-based businesses for customer retention and development. In our context, we find subscription programs are broadly effective in lifting sales and enhancing customer engagement (e.g., breadth and depth of purchases in products and categories). While we study one subscription program launched by a single firm, the insight that the effect of subscription goes beyond the economic benefits of the program is not limited to the structure of our focal program. Our results also enable us to derive recommendations related to the design of subscription programs. For example, given the sensitivity to sunk costs, it may benefit companies to make the initial payment more salient after customers become members. Our suggestion is in line with past work showing that making prices salient can make members consume a service on a more consistent basis (e.g., Gourville and Soman 2002). As members' enhanced status encourages purchases,

managers can emphasize the exclusiveness of the program to boost sales. Our analyses also contribute to the ongoing debate concerning the amplified friction in the retail industry due to the rise of subscription programs (e.g., Amazon Prime). Our results suggest that firms are able to lock in customers with subscription programs by creating a sunk cost fallacy which may potentially lead to increased market concentration.

As our research is the first attempt to identify the effect of subscriptions in the retail area on customer behavior, naturally there are limitations that should be acknowledged and addressed in future research. First, as our study focused on the effect of a subscription program in a given firm, it is likely that some of our findings could reflect the customer base and product categories of our partner firm. The subscription period is also reasonably long (one year) and early termination was not allowed, so sunk cost is prominent. The subscription program examined in this research is an online-only program, while members in other subscription programs could benefit at both online and offline channels (e.g., Amazon, Barnes & Noble). With that in mind, we hope our approach provides a framework for further studies on subscription programs with various pricing schemes or framing (e.g., programs with more varying time windows for subscriptions) in other product categories and across channels. Second, while we examined customer behavior in the first year of subscription rollout, it could be interesting to measure the long-term (e.g., 3 years) effect of subscriptions on customer behavior beyond the first year of subscription rollout. Finally, with the relevance and popularity of subscription-based businesses, it is possible that many more companies will have their own subscription programs. With competition in play, the effect of a subscription program on customer engagement and purchase remains unclear. We hope that our work will inspire further studies to deepen our understanding in this nascent and important area of research.

CHAPTER 5 : Conclusion

In this dissertation, we study customer behavior and firm strategy under two types of assortment expansions. Chapter 2 and 3 study customers' price sensitivity and retailer's pricing strategy when a retailer expands its product assortment. Chapter 4 evaluates the impact of subscription programs on customer purchases.

We model demand using discrete choice and econometric models. Chapters 2 and 3 explicitly model customers' multi-category purchases in a bundle utility framework. Counterfactual simulations based on the model enable us to study the effect of various constructs and policies. In Chapter 4, we leverage a quasi-experiment design to non-parametrically infer counterfactual outcomes and identify the treatment effects of a subscription program. While these two types of demand models build on different assumptions, they provide good predictions at the individual level, which allows us to empirically test marketing theories.

We find that customer behavior is endogenously determined by retailers' assortment strategy in both contexts. In Chapters 2 and 3, we demonstrate that the price sensitivity of an existing category may increase after assortment expansion. In Chapter 4, we find subscription programs induce a sunk cost fallacy and lead to a large increase in customer purchases.

Our work shed light on a variety of marketing contexts. We provide novel evidence against the conventional belief that "more is better." We are also among the first to document sunk cost fallacy in the field using observational data. These findings enable us to derive recommendations for marketing practice. For example, as price sensitivity depends on the assortments, a retailer should update the demand forecast and re-optimize price levels after assortment expansion. Otherwise, it might have a loss in expected profit. More broadly, retailers should also take into account the spill-over effect of expansion when managing the assortments. Our analyses also speak to the ongoing debates concerning the amplified frictions in the retail industry. For instance, our results suggest that the rise of subscription programs may potentially lead to increased market concentration as they can increase the

switching costs.

Our work can be extended in several aspects in the future. First, the demand model in Chapter 2 focuses on primary demand (purchase incidence). Future work may investigate the change in the price sensitivity in a unified framework that models both purchase incidence and quantity. Second, both of our works leverage a unique natural experiment setting. Future research could show the robustness of these effects in other settings.

In sum, we study customer behavior and firm strategy under assortment expansion. We hope that our work will inspire further studies to deepen our understanding in this area.

APPENDIX

A. Appendix for Chapter 2

A.1. Proof of Proposition 1

To solve $|e_A(0)| > |e_A(s_0)|$, i.e. $\frac{\partial Q_A(0)}{\partial p_A} < \frac{\partial Q_A(s_0)}{\partial p_A}$, it is equivalent to solve $\frac{\partial^2 Q_A(s_0)}{\partial s \partial p_A} > 0$.

We note that $\frac{\partial Q_A(s)}{\partial p_A} = -\beta Q_A(s) (1 - Q_A(s))$. Then,

$$\begin{aligned} \frac{\partial^2 Q_A(s_0)}{\partial s \partial p_A} &= -\beta (1 - 2Q_A(0)) \cdot \frac{\partial Q_A(0)}{\partial s} \\ &= -\beta (1 - 2Q_A(s_0)) \cdot \frac{\exp(v_{AB}) (\exp(v_A) + \exp(v_B) + 1)}{(\exp(v_{AB}) + \exp(v_A) + \exp(v_B) + 1)^2}. \end{aligned}$$

Hence, the change in price sensitivity is determined by the sign of $1 - 2Q_A(s_0)$. Equivalently, the demand for category A is more price sensitive if $v_A < \log \frac{\exp(v_B)}{\exp(v_B + \Gamma_{AB} - s_0) + 1}$.

A.2. Proof for Lemma 1

First, we show that $\frac{\partial Q_A(s)}{\partial s} < 0$ if $Q_{AB} > \frac{Q_A}{1 - Q_A} (Q_{BC} - Q_{ABC})$.

$$\begin{aligned} &\frac{\partial Q_A(s)}{\partial s} \\ &= - \frac{\exp(v_{AB}) + \exp(v_{ABC})}{1 + \exp(v_{ABC}) + \exp(v_{AB}) + \exp(v_{BC}) + \exp(v_{AC}) + \exp(v_A) + \exp(v_B) + \exp(v_C)} \\ &\quad + Q_A \cdot \frac{\exp(v_{ABC}) + \exp(v_{AB}) + \exp(v_{BC})}{1 + \exp(v_{ABC}) + \exp(v_{AB}) + \exp(v_{BC}) + \exp(v_{AC}) + \exp(v_A) + \exp(v_B) + \exp(v_C)} \\ &= -(1 - Q_A)(D_{ABC} + D_{AB}) + Q_A D_{BC} \\ &= -(1 - Q_A)Q_{AB} + Q_A(Q_{BC} - Q_{ABC}) < 0. \end{aligned}$$

This gives $Q_{AB} > \frac{Q_A}{1 - Q_A} (Q_{BC} - Q_{ABC})$.

We then show that $\frac{\partial Q_A(s)}{\partial s}$ is decreasing in Γ_{AB} . The above equation can be equivalently

written as

$$\frac{\partial Q_A(s)}{\partial s} = -Q_{AB}^2 + (D_{BC} - D_A - D_{AC} - 1)Q_{AB} + (D_A - D_{AC})D_{BC}.$$

Then

$$\frac{\partial^2 Q_A(s)}{\partial \Gamma_{AB} \partial s} = -\frac{\partial Q_{AB}}{\partial \Gamma_{AB}} \cdot (2Q_{AB} + D_A + D_{AC} + 1 - D_{BC}) < 0.$$

The inequality holds because $\frac{\partial Q_{AB}}{\partial \Gamma_{AB}} > 0$ and $2Q_{AB} + D_A + D_{AC} + 1 - D_{BC} > 2Q_{AB} + D_A + D_{AC} > 0$.

Combining the above results, $\frac{\partial Q_A(s)}{\partial s} < 0$ if $\Gamma_{AB} > \bar{\Gamma}_{AB}$ where $\bar{\Gamma}_{AB}$ solves $Q_{AB} = \frac{Q_A}{1-Q_A}(Q_{BC} - Q_{ABC})$, $Q_A(s) < Q_A(0)$.

A.3. Proof of Proposition 2

Note that $\frac{\partial Q_A(s)}{\partial p_A} = -\beta Q_A(s)(1 - Q_A(s))$ still holds. Then,

$$\frac{\partial^2 Q_A(s)}{\partial s \partial p_A} = -\beta(1 - 2Q_A(s)) \cdot \frac{\partial Q_A(s)}{\partial s} > 0$$

when $Q_A(s) < \frac{1}{2}$ and $\frac{\partial Q_A(s)}{\partial s} > 0$, or $Q_A(s) > \frac{1}{2}$ and $\frac{\partial Q_A(s)}{\partial s} < 0$. Combining the results from Lemma 1, we prove Proposition 2.

A.4. Empirical Tests for Selections

We estimate the difference in sales of grocery stores with and without liquor in month m using the following specification:

$$Y_{st} = \Sigma_m \tau_m \cdot I(t = m) \cdot \text{Treated}_s + Z_{st}\beta + \alpha_s + \gamma_t + \epsilon_{st},$$

where $I(t = m)$ are dummies for month m , with January 2012 being the baseline. Treated_s indicates whether store s started to carry liquor in June 2012. The parameter τ_m thus captures the difference in sales of stores with and without liquor relative to the baseline in month m . As 99% of grocery stores in our data started to carry liquor post privatization,

in this analysis, we use the stores in the same chains in the state of Oregon as controls. We estimate the sales difference four months before the privatization, i.e., February 2012 and May 2012. Table 23 reports the results from the regression. We find the estimates are mostly insignificant. These results indicate that the stores started to carry liquor in 2012 and those who did not have statistically comparable time trends prior to the privatization.

Table 23: Empirical Tests for Parallel Time Trends

Month	Effect
Feb 2012 (\$)	7.24 (13.56)
March 2012 (\$)	23.48*** (10.66)
April 2012 (\$)	-12.91 (9.27)
May 2012 (\$)	-3.85 (11.93)

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors in parentheses.

A.5. Estimation Details

First, we complete our model by specifying the prior distributions. We use standard hyperpriors for the population parameters. Specifically, we let $\bar{\Theta}$ to have a prior of $MVN(0, 2I)$. For the prior of Σ_{Θ} , we first redefine Σ_{Θ} using LDL decomposition, i.e. $\Sigma_{\Theta} = \tau\Omega\tau'$, where Ω is a correlation matrix and τ is the vector of coefficient scales (Barnard et al., 2000). τ is given a prior of $MVN(0, I)$. The correlation matrix Ω is given an LKJ prior with a shape parameter of 4 (Lewandowski et al., 2009). Finally, the priors of $\Gamma, \eta, \lambda, \phi$ are assumed to be standard normal distributions.

To make the model tractable, we restrict attention to bundles of up to five categories which cover more the 95% of the co-purchase. We estimate the model using a Hamiltonian Monte Carlo (HMC) algorithm within Stan (Carpenter et al., 2017). We run 4 independent chains, each from a different and dispersed starting value. We run each chain for 1000 iterations, using the first 200 iterations as burn-in and the last 800 iterations for analysis. We check convergence using the poerior scale reduction factor (Gelman et al., 1992) and make sure

that the statistics are less than 1.2 for all parameters.

A.6. Parameter Recovery

We use a simulation to ensure that our specified model is identified. We simulated data for the same number of individuals and shopping trips as in the observed data. We estimate the model using HMC in stan.

Table 24 shows the parameter values used to simulate the data and the estimated mean and the 95% credible intervals of the parameters. The 95% intervals contain the true means for our parameters, suggesting that our model is identified.

Table 24: Parameter Recovery

Parameter		Main effect			Interaction with household income			Interaction with liquor store density		
		True	Estimated	95% CI	True	Estimated	95% CI	True	Estimated	95% CI
Price coefficient	β	-1	-1.01	[-1.08, -0.96]	-1	-1.06	[-1.16, -0.96]	1	1.04	[0.99, 1.09]
Transportation cost	s	2	1.98	[1.73, 2.23]	-1	-0.99	[-1.29, -0.69]	1	0.99	[0.84, 1.14]
Category intercept: Liquor	α	1	1.20	[-0.02, 0.42]	-0.5	-0.55	[-1.09, -0.04]	-0.5	-0.66	[-1.20, -0.19]
Dry grocery		1	1.04	[0.83, 1.27]	-0.5	-0.55	[-1.09, -0.03]	-0.5	-0.44	[-0.96, 0.05]
Frozen foods		1	0.99	[0.77, 1.21]	-0.5	-0.32	[-0.86, 0.17]	-0.5	-0.21	[-0.74, 0.26]
Dairy		1	1.22	[1.01, 1.43]	-0.5	-0.64	[-1.17, -0.12]	-0.5	-0.55	[-1.07, -0.08]
Deli		1	1.05	[0.84, 1.26]	-0.5	-0.38	[-0.91, 0.12]	-0.5	-0.41	[-0.94, 0.08]
Packaged meat		1	1.13	[0.93, 1.35]	-0.5	-0.5	[-1.02, -0.01]	-0.5	-0.41	[-0.94, 0.06]
Alcoholic beverages		1	1.03	[0.95, 1.10]	-0.5	-0.32	[-0.89, 0.18]	-0.5	-0.47	[-0.99, 0.01]
Bundle utility: Dry grocery	Γ	1	0.96	[0.12, 1.77]	-0.5					
Frozen foods		1	0.84	[-0.01, 1.66]						
Dairy		1	1.06	[0.20, 1.88]						
Deli		-1	-0.79	[-1.65, 0.01]						
Packaged meat		-1	-1.34	[-2.19, -0.51]						
Alcoholic beverages		-1	-0.92	[-1.74, -0.13]						

B. Appendix for Chapter 4

B.1. Placebo Tests

We defined a placebo treatment on members six months prior to when the actual subscription took place and estimated the effect of such a placebo treatment on customer purchases using a DD model on all weighted pre-subscription data. If the parallel trend assumption holds, we should expect null effects. Table 25 reports the results from the placebo tests. These results indicate that the members and the matched non-members have statistically comparable pre-treatment time trend.

Table 25: Placebo Tests

	Placebo Effect
Purchase amount (\$)	1.50 (1.49)
Purchase frequency	0.03 (0.03)
Basket size (\$)	1.20 (2.93)

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses. Placebo tests on the variety metrics were left out. By design, purchase amount of known products (and categories) equals to the monthly purchase amount before subscription and the purchase amount of new products (and categories) remains unchanged (at zero) before subscription.

B.2. Operationalization of Store-level Price

In the main analysis, we construct the store-level price (Price_{it}) for customer i in month t as the weighted average price of a basket of products, which is defined as follows:

$$\text{Price}_{it} = \frac{\sum_{j \in J_t} P_{gjt} \cdot Q_{jt}}{\sum_{j \in J_t} Q_{jt}}$$

where P_{gjt} is the minimum price of product j paid by group g (members or non-members) in month t , Q_{jt} is the quantity of product j purchased by members, and J_t is the set of products that were purchased by both members and non-members in month t . Our group- and store-level price variable provides appropriate information on the level of prices that members were exposed to due to the subscription program and its variation on a monthly basis.

With this operationalization, we capture the upper bound of the discounts offered to members as the products which members received deeper discounts for were purchased more often by them and are weighted more so in our formulation. By doing so, we obtain a conservative estimate of the non-economic effect of subscription.

To evaluate the robustness of our results, we replicated the main analyses using a few alternative prices. First, we define the group-level price for a product as the average price paid by the group for the product. We find the results are quite similar to those reported in the main text (19.81).

We also tested an alternative definition for the basket of products. For example, Dubé et al. (2018) calculate a store-level price index and impute shelf price as non-promotion price when sales quantity is zero and shelf price is not observed. Following the literature, we construct a basket of products that include all the products offered by the website and impute missing prices with listed prices. We find the price variation across members and non-members was masked by the noise through the imputation of price information, probably due to a large number of products offered at the website and sparse price information in our e-

commerce context. Our choice of the basket of products in the main analysis (those products purchased by both members and non-members) result in a store-level price that captures the price variation across members and non-members well while ensuring a sufficient number of products are included for the analysis.

We also tested alternative definitions for the weights. For example, we define the weights as quantity based on purchases by both members and non-members. The non-economic effect estimates are qualitatively similar when alternative weights are specified (22.56).

Finally, we specified an alternative price variable at the individual level. We use a two-step procedure following Thomassen et al. (2017):

Step 1: We obtain group-level product prices as the minimum price paid by the group and construct group-level category prices using weighted-average product prices where the weights are total quantity purchased by both members and non-members, i.e., $P_{gct} = \frac{\sum_{j \in J_{ct}} P_{gjt} \cdot Q_{jt}}{\sum_{j \in J_{ct}} Q_{jt}}$, where P_{gjt} is the minimum price of product j paid by group g (members or non-members) in month t , Q_{jt} is the quantity of product j purchased by both groups, and J_{ct} is the set of products in category c purchased by both groups in month t .

Step 2: Based on category-level prices, we construct store-level prices using individual-specific weights. The weights are chosen to be an individual's purchase shares of the categories throughout the data (two year) period, i.e., $P_{it} = \frac{\sum_c P_{gct} \cdot Q_{ic}}{\sum_c Q_{ic}}$. The resulting price variable varies at the individual-month level and reflects each customer's idiosyncratic preference for the product categories.

The estimated non-economic effects (23.31) is comparable to the one reported in the manuscript.

B.3. Log-linear Demand Model

In Appendix B.3, we show that our demand model is consistent with utility maximization under Cobb-Douglas preference (e.g., Sato 1972).

Consider a customer with a budget I and consumes Y units of products in the focal company and Z units of the outside product. The price for the inside product is given by p and the price for the outside product is normalized to 1.

The customer has Cobb-Douglas preference, that is, her utility from (Y, Z) is given by:

$$U = Y^\alpha Z^{1-\alpha}$$

And the customer has the following budget constraint:

$$pY + Z = I$$

Then the utility-maximizing customer's demand (in the focal company) follows:

$$\log(Y) = \log(\alpha) + \log(I) - \log(p).$$

Given the above equation, if there were an increase in the customer's budget by ΔI , the resulting demand function would be as follows:

$$\log(Y) = \log(\alpha) + \log(I + \Delta I) - \log(p). \tag{A.1}$$

Thus, any changes in the budget are included within the logarithmic term. By doing so, the resulting demand model remains consistent with the underlying utility maximization. In our context, the increase in the budget for members was due to the monthly gift card. With that being in the case, the amount of gift card is included within the logarithmic term.

To work with empirical data, we relax the constraint that the income and price elasticities

are unity. Demand functions of similar form have also been used to study consumer demand (e.g., Berndt et al. 1977, Young 2012, 2013, Aguiar and Bils 2015) and the demand for water and land (e.g., Gaudin et al. 2001, Griffin and Chang 1991). We also generalize the model to use outcome variables other than log quantity as the dependent variable. In our main analysis, for the ease of interpretation, we use the level of demand as the dependent variable. Finally, we include customer- and month-fixed effects and estimate our empirical model.

B.4. Estimation

We describe the details for estimation, including the implementation details for GRF and the construction of the average treatment effect estimator.

B.5. Implementation Details for GRF

Our goal is to recover the heterogeneous parameters in Equation (4.2) conditional on covariates x . Let $\theta(x) = \{\tau(x), \beta_1(x), \beta_2(x)\}$ denote the parameters of interest. The GRF algorithm takes a three-step procedure. To accommodate the fixed effects and enhance the robustness of the results, we first pre-process the data. We next build a random forest that defines the weighted set of neighboring customers for each member. Finally, the treatment effects are obtained by estimating Equation (4.2) on the weighted sets. Below we describe each step in detail.

To accommodate the fixed effects, we first demean the variables (outcome variable, membership indicator, and marketing mix variables) in Equation (4.2). To enhance the robustness the results, following Athey et al. (2019), we further implement an orthogonalization procedure: we residualize the (demeaned) variables by the covariates x using separate trained regression forests. The final GRF is trained on the residuals instead of the original variables.

To build the random forest, we grow a decision tree through iteratively partitioning the data into subgroups (leaves). In each iteration, the algorithm chooses the covariate and the best cutoff for dividing a parent node P into two child nodes C_1 and C_2 by optimizing the Δ -criterion:

$$\Delta(C_1, C_2) = \frac{n_{C_1}n_{C_2}}{n_P^2} \left| \hat{\theta}_{C_1} - \hat{\theta}_{C_2} \right|^2, \quad (\text{A.2})$$

where $\hat{\theta}_{C_1}$ and $\hat{\theta}_{C_2}$ are estimation results from Equation (4.2) computed in the child nodes C_1 and C_2 . n_{C_1} , n_{C_2} , and n_P are number of observations in the child nodes and parent nodes, respectively. The Δ -criterion approximates the expected squared error from Equation (4.2) and also measures the increase in the heterogeneity of the estimated treatment effects. The

algorithm continues to partition the data until there is at least k treated and controls in each leaf.

As a single tree is likely to overfit the data, an ensemble of B trees is generated. The b th tree is constructed using a random subsample without replacement containing n_b observations from a total of N observations in the data. For each tree, a random set of proportion p of the covariates can be potentially used to form the splits. The number of trees B , the minimum number of treatment and control observations in each leaf k , the subsample size n_b , and the size of the set of covariates used to build each tree p are the hyper-parameters of a forest which we choose by cross-validation. Finally, in the context of panel data, we account for clusters at the individual level in the sampling as well as estimation process.

Given the forest, we can define for each member a weighted set of its neighboring customers. Specifically, for a member with covariates x , the weights are the frequency with which each customer falls into the leaves that contains x .

With the weighted set of neighboring customers defined for each member, the individual-level treatment effect is estimated by fitting Equation (4.2) using the weighted set of observations:

$$\theta(X_i = x) = \underset{\theta=\{\tau, \beta_1, \beta_2\}}{\operatorname{argmin}} \sum_{i=1}^N \sum_{t=1}^T w_i(x) [Y_{it} - \tau(x)M_{it} - \beta_1(x) \log(P_{it}) - \beta_2(x) \log(B_i + G_{it})]^2,$$

where $w_i(x)$ measure the similarity of customer i and x . Y_{it} is the outcome variable of interest. M_{it} is an indicator for membership status that equals one if customer i is a member in month t and zero otherwise. P_{it} is the price encountered by customer i in month t . B_i is the baseline spend of customer i , and G_{it} is the amount of gift card offered to customer i in month t . Y_{it} , M_{it} , $\log(P_{it})$, and $\log(B_i + G_{it})$ are residualized by the fixed effects and covariates x .

We use monthly transaction data over a 24-month period. Of these, the first 12 months

are prior to their subscription. We exclude customer purchases in the month of adoption to avoid any potential simultaneity bias with the adoption itself.

We choose the hyper-parameters of the model by cross-validation. We optimize on the three core hyper-parameters: node size (stopping criterion), sample fraction, and number of splitting covariates in a tree. For node size, we consider 1, 2, 3, 4, 5, 10, 15, and 20. For sample fraction, we consider 0.1, 0.2, 0.3, 0.4, and 0.5. For number of splitting covariates, we consider \sqrt{n} , $2\sqrt{n}$, $n/3$, $n/2$, where n is the number of covariates ($n = 72$). We use cross validation to choose a set of hyper-parameters that gives the smallest out-of-bag error.

B.6. Doubly Robust Estimator

In order to obtain the treatment effect across members and examine the average effectiveness of the program, we construct a doubly robust average treatment effect estimator.

One naïve estimator for the average treatment effect can be obtained by simply averaging individual-level effects from the GRF, i.e., for n customers with covariates X_i , $i = 1, \dots, n$, $\bar{\tau} = \frac{1}{n} \sum_{i=1}^n \hat{\tau}(X_i)$. The idea of the doubly robust estimator is to combine the individual-level estimates from the GRF and a regression-based prediction such that it is robust to misspecification of either the matching model or the regression model (Chernozhukov et al. 2018). Specifically, the doubly robust estimator augments the naïve estimator with the residuals from a regression forest as a bias-correction term and is defined as:

$$\bar{\tau}^{DR} = \frac{1}{n} \sum_{i=1}^n \hat{\tau}(X_i) + \sum_{i=1}^n \sum_{t=1}^T \frac{(Y_{it} - \hat{Y}_{it})(M_{it} - \hat{M}_{it})}{(M_{it} - \hat{M}_{it})^2} \quad (\text{A.3})$$

where \hat{Y}_{it} and \hat{M}_{it} are predictions of Y_{it} and M_{it} from regression forests. The variance of this estimator, as is standard for linear models, is defined as:

$$\text{Var}(\bar{\tau}^{DR}) = \frac{1}{(nT - 1)nT} \sum_{i=1}^n \sum_{t=1}^T \left[\frac{(Y_{it} - \hat{Y}_{it})(M_{it} - \hat{M}_{it})}{(M_{it} - \hat{M}_{it})^2} \right]^2. \quad (\text{A.4})$$

B.7. Identification of the DD Model with Covariates

We used a simulation study to understand how different patterns in purchase data can help identify the parameters of the DD model in Equation (4.2). As described in Section 4, the price coefficient (β_1) is identified by the month-to-month variation in purchase amount and store-level price. The parameter β_2 is identified by the difference of the unexplained increase in purchase amount upon subscription across members who differ in terms of the baseline spend. The parameter τ is identified by the remaining change in member purchases upon subscription.

For the purpose of illustration, we simulated the purchase patterns of two members with different levels of baseline spend and one non-member under various sets of parameters. Specifically, we choose two levels (low versus high) of the baseline spend among members, and set \$12 for the low-spend member and \$20 for the high-spend member, and \$6 for the non-member. We use the values of the simulation parameters which are chosen to mimic the value found in our real data. The variation in prices (between the two groups and over time) mimics our data while the levels of prices are normalized by the average price for the non-members. All the time-fixed effects are assumed to be zero and customer-fixed effects are set to be each customer's baseline spend. Further simulations not reported suggest that parameter recoverability is robust.

Figures 14 to 17 show the purchase patterns over a 24-month period, excluding the month of adoption (April 2016), under different sets of parameters. Figure 14 shows the purchase patterns when all three parameters are set to 0, i.e., $\tau = \beta_1 = \beta_2 = 0$. When customers are insensitive to economic benefits (e.g., price, gift card) and there is no non-economic effect of the program, as expected, purchase patterns are constant at the level of baseline spend and do not change over the data period. Figure 15 shows the purchase patterns when $\tau = 0$, $\beta_1 = -3$ and $\beta_2 = 0$. When customers are sensitive to price but do not respond to other types of economic benefits (e.g., gift card) and there is no non-economic effect, we find there is a month-to-month variation in customer purchases that are correlated with the changes in

price due to discounts. The purchase patterns also vary across customers because members obtain, for example, member-exclusive discounts post subscription, which make members purchase more than non-members who could not obtain such offers. Figure 16 shows the purchase patterns when $\tau = 0$, $\beta_1 = 0$, and $\beta_2 = 3$. This is the case when customers are responsive to gift card but do not respond to price and there is no non-economic effect. With parameter $\beta_2 > 0$, members increase their purchases upon subscription. Importantly, the magnitude of the increase differs depending on the member's baseline spend. Finally, Figure 17 plots the case when $\tau = 18$, $\beta_1 = 0$, $\beta_2 = 0$. A positive non-economic effect results in an increase in purchase among members at the time of their subscription. And the magnitude of the increase is common across all members.

In summary, parameter recoverability is robust. We exploited the panel structure of our data and heterogeneity in the baseline spend. The parameters β_1 and β_2 are identified by the month-to-month variation in customer purchases and heterogeneity in the pre-subscription baseline spend among members, respectively. The parameter τ is identified by the remaining change in member purchases upon subscription.

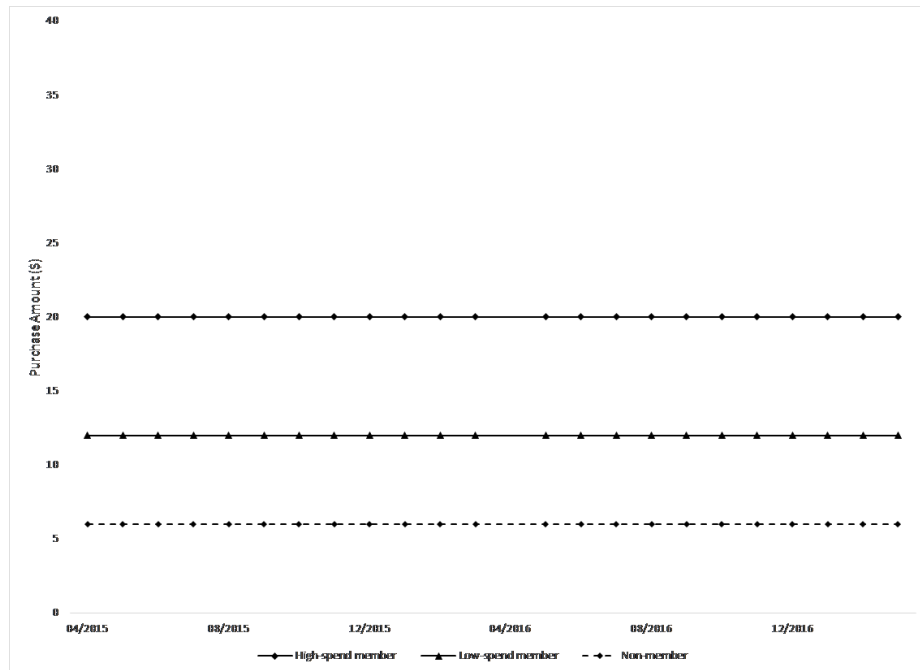


Figure 14: Customer Purchases when $\tau = 0$, $\beta_1 = 0$, $\beta_2 = 0$

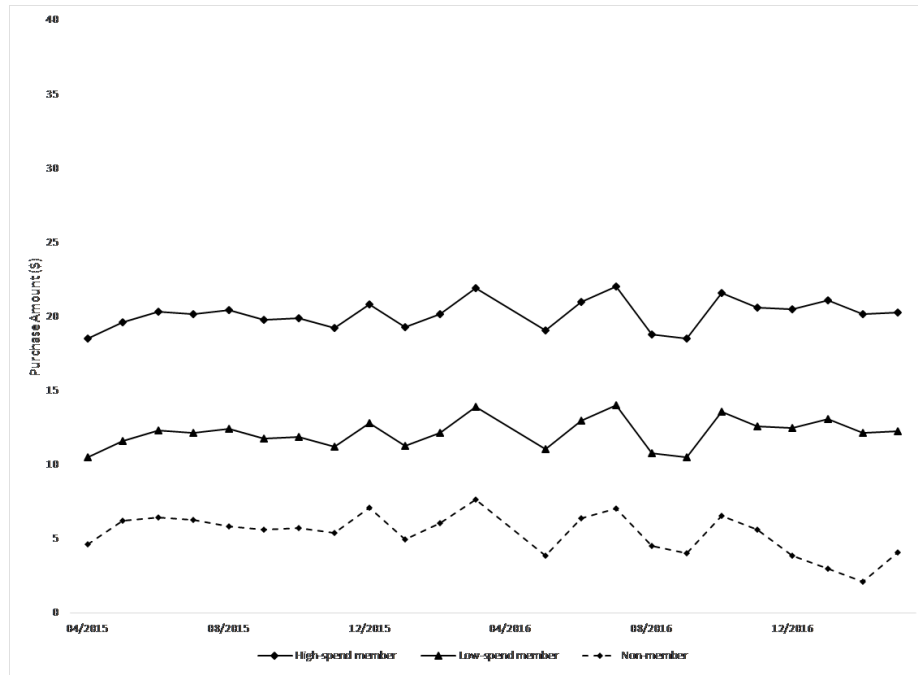


Figure 15: Customer Purchases when $\tau = 0, \beta_1 = -3, \beta_2 = 0$

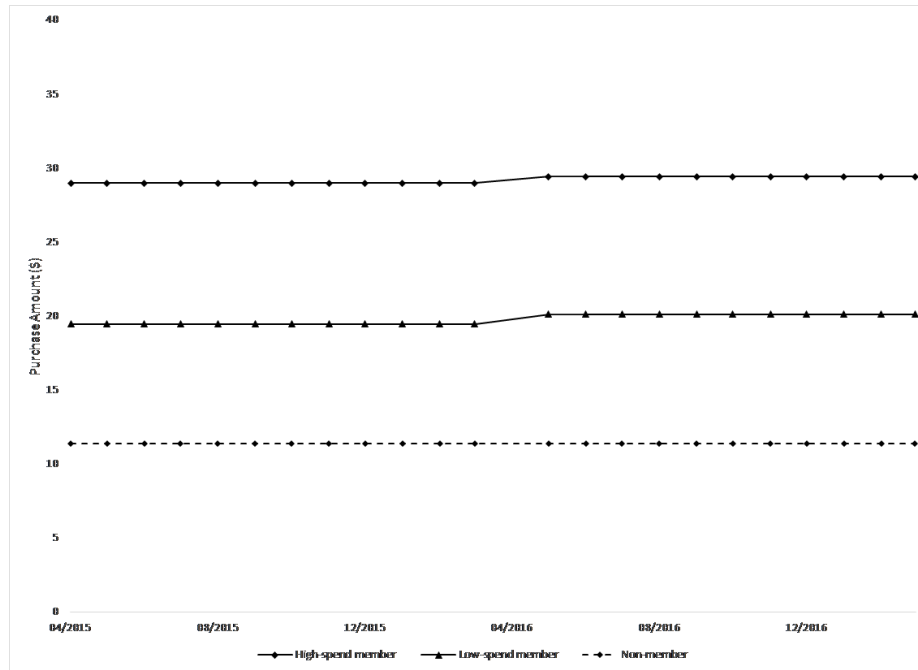


Figure 16: Customer Purchases when $\tau = 0, \beta_1 = 0, \beta_2 = 3$

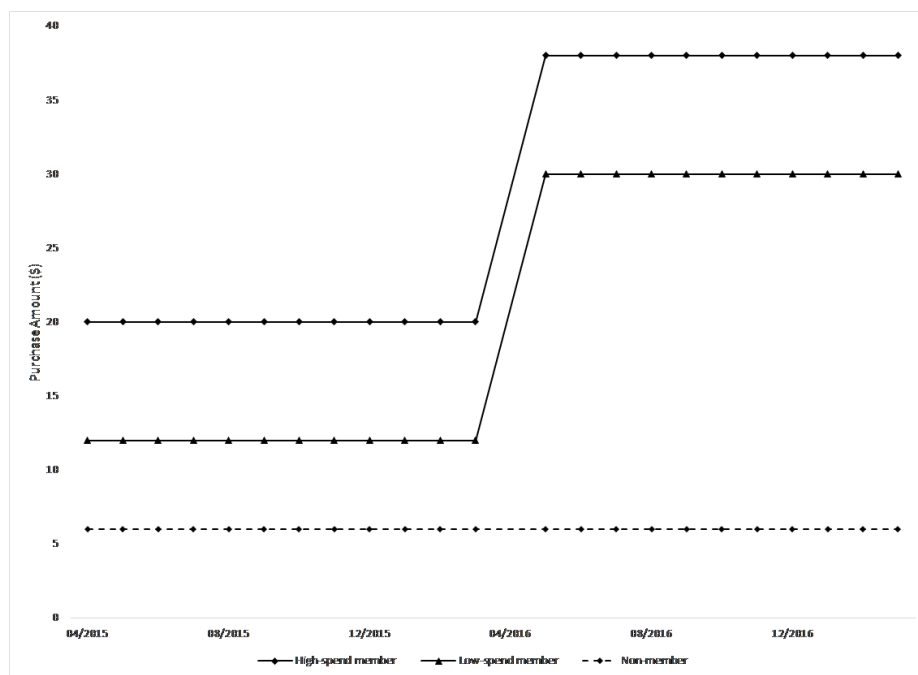


Figure 17: Customer Purchases when $\tau = 18$, $\beta_1 = 0$, $\beta_2 = 0$

B.8. Identification of Heterogeneous Treatment Effects

The identification of heterogeneous treatment effects relies on the same source of variation in the data as those used to identify the DD model. However, one major challenge in estimating heterogeneous treatment effects is that the individual-level treatment effects are estimated on a weighted (matched) sample which is potentially of a smaller size than the full sample. If there were not enough variation in these weighted samples, the individual-level treatment effects would be estimated imprecisely. Hence, we used a simulation study to show that there is sufficient variation in our data to identify the heterogeneous treatment effects.

To demonstrate the recoverability of the parameters, we simulated a data to mimic our data set in size and nature (e.g., number of members and non-members, length of the data for each customer, marketing mix, covariates). We also allow the model parameters to be non-linear functions of the covariates. The fixed effects are set to be zero. Finally, the error terms are drawn from the standard normal distribution to generate the outcomes.

We apply our proposed estimation method to estimate heterogeneous treatment effects. Figures to compare the estimates with the true parameters across all individuals. We find the recovery of the individual-level estimates is robust and suggests that there is sufficient variation in the data to identify heterogeneous treatment effects.

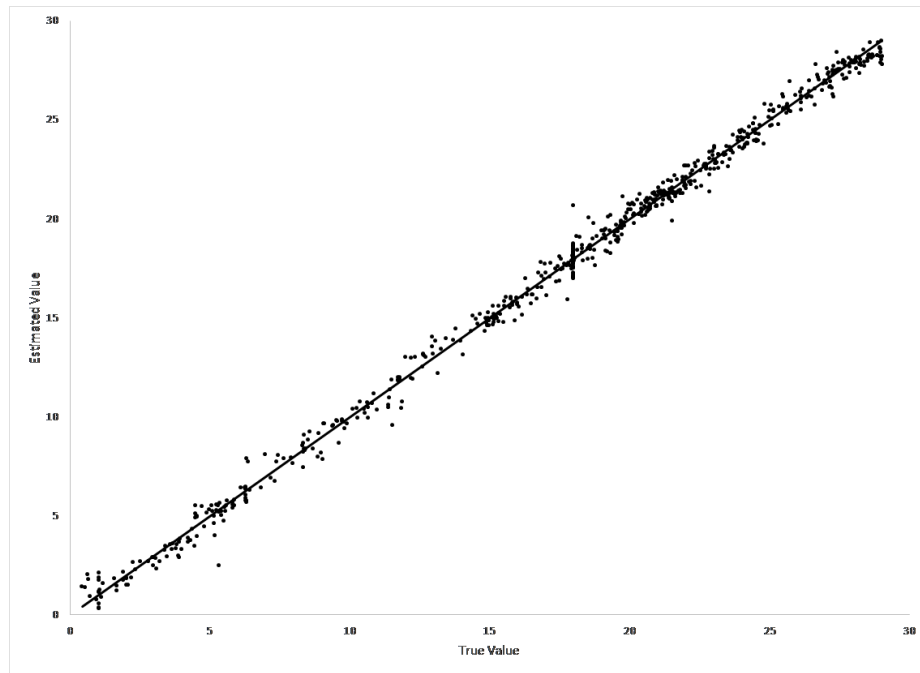


Figure 18: Parameter Recovery of τ

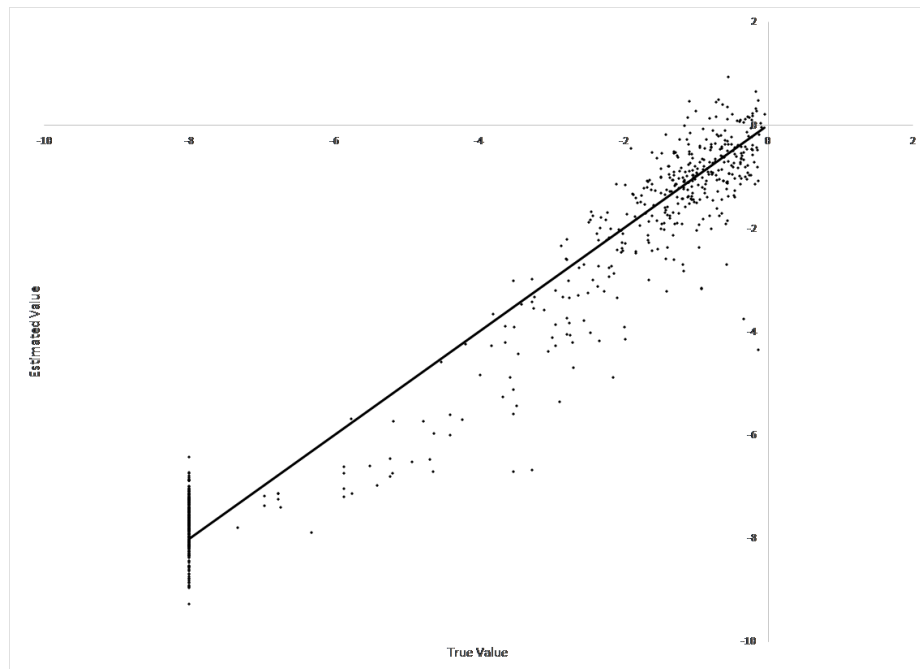


Figure 19: Parameter Recovery of β_1

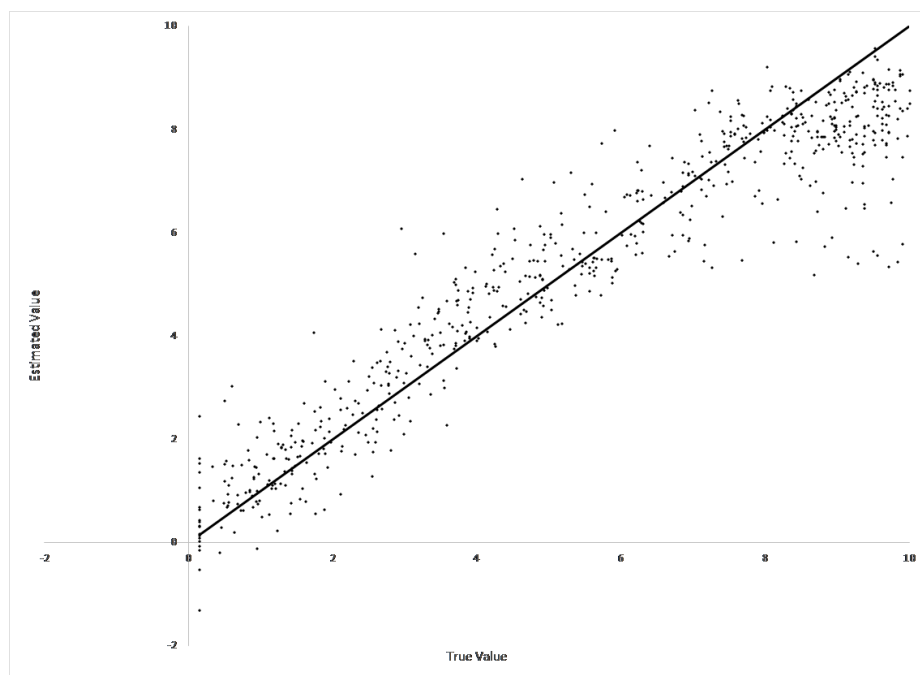


Figure 20: Parameter Recovery of β_2

B.9. Average Treatment Effects with $\log(Y_{it})$

Tables 26 and 27 report the average treatment and non-economic effects of subscription on customer purchases, respectively, where the outcome measure is transformed with natural log.

Table 26: Treatment Effects with $\log(Y_{it})$

	All months	Month 2	Months 3-4	Months 5+
Purchase amount (\$)	1.57*** (0.05)	2.05*** (0.13)	1.80*** (0.08)	1.50*** (0.27)
Purchase frequency	1.47*** (0.11)	1.87*** (0.13)	1.57*** (0.12)	1.67*** (0.15)
Basket size (\$)	-0.34*** (0.14)	-0.17* (0.09)	-0.24*** (0.05)	-0.33*** (0.11)
Variety (\$)				
Purchase amount in known categories	1.82*** (0.05)	2.08*** (0.11)	1.86*** (0.08)	1.63*** (0.06)
Purchase amount in new categories	0.19*** (0.02)	0.15*** (0.03)	0.24*** (0.03)	0.18*** (0.02)
Purchase amount in known products	0.08** (0.04)	0.06 (0.04)	0.05 (0.04)	0.05 (0.04)
Purchase amount in new products	1.92*** (0.04)	2.26*** (0.09)	2.13*** (0.06)	1.77*** (0.04)

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses. Customer purchases in the month of adoption (month 1) were excluded in the analysis.

Table 27: Non-economic Effects with $\log(Y_{it})$

	All months	Month 2	Months 3-4	Months 5+
Purchase amount (\$)	1.34*** (0.05)	1.63*** (0.11)	1.49*** (0.08)	1.11*** (0.05)
Purchase frequency	0.47*** (0.01)	0.54*** (0.04)	0.54*** (0.02)	0.40*** (0.01)
Basket size (\$)	-0.24 (0.17)	-0.16* (0.09)	-0.24*** (0.04)	-0.29*** (0.12)
Variety (\$)				
Purchase amount in known categories	1.32*** (0.05)	1.58*** (0.11)	1.46*** (0.06)	1.13*** (0.07)
Purchase amount in new categories	0.15*** (0.03)	0.12** (0.05)	0.21*** (0.02)	0.17*** (0.04)
Purchase amount in known products	0.07** (0.03)	0.11*** (0.04)	0.04 (0.03)	0.05 (0.05)
Purchase amount in new products	1.42*** (0.04)	1.64*** (0.08)	1.51*** (0.05)	1.27*** (0.05)

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Robust standard errors appear in parentheses. Customer purchases in the month of adoption (month 1) were excluded in the analysis.

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